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THE OPTICAL THEORY OF PORTABLE DAYLIGHT SIGNALLING LAMPS

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THE *ideal* signalling lamp for use by day should fulfil the following conditions: The whole equipment should be easily carried by one man from place to place—the actual signalling lamp should be easily aimed and operated by one hand. The beam projected should be of uniform and great intensity within a reasonable angle to allow for errors of aim; and of zero intensity for all angles of divergence outside that tolerance. It should be provided with a good optical sight to ensure accurate aim, and the source should be standardised in some way at the focus of the projector, so that the operator cannot interfere with it or throw the beam and sight out of alignment. The device for interrupting the beam should be easily operated by one finger, have the minimum of movement and noise, and enable the beam either to occult from maximum intensity or to flash out again to maximum intensity in less than a tenth of a second.

Now it is difficult to satisfy all these conditions, but it will be the purpose of this paper to deal with certain simple geometrical methods of dealing with the purely optical problems presented, viz. to afford a beam of maximum intensity within definite angular limits from apparatus of minimum size and weight.

In the first place, it is necessary to point out that the stimulus to the eye of the receiver of the message is, in really efficiently designed apparatus, strictly proportional to the product of the area of the clear aperture of the lamp into the intrinsic brightness of the source. If suitably designed and focussed, what the eye sees is the whole aperture of the lamp filled with a brightness equal (except for transmission losses) to the brightness of the source. Of course, signal lamps

are operative by day at ranges far beyond those at which the eye can see the lamp as a *disc* of light, but the fact remains that the increase in candle power as contrasted with the naked source is only due to the increase in apparent size of the source effected by the optical device employed in the lamp. The optical device cannot make the source look brighter; it only makes it look larger. In all that follows this distinction as to what the eye really sees will be ignored and for scientific exactitude the reader can imagine the image as being transmitted to the eye by a really good telescope.

It will be convenient to define the apparent shape and size of any small incandescent source, corresponding to any particular view point, as the actual shape and area of a flat, cut out sheet which, held with its plane perpendicular to the line of vision, just completely obscures the source as seen from that view point.

Now the condition of portability imposes a definite upper limit for the size of lamp and in all that follows it will be assumed that a lamp of known maximum allowable clear aperture is being used. It is, therefore, at once evident that a source must be chosen of as high an intrinsic brightness as possible, and that the optical device employed in the lamp must be so related to the source that the receiver of the message sees the whole clear aperture of the lamp as filled with a flash of that intrinsic brightness. This is termed "complete flashing"; "partial flashing" means that the receiver sees certain parts of the clear aperture as either without any flash or as only filled with a *coloured* flash, due to the optical device failing over those zones to bend *all* the useful light energy into the receiver's eye.

The subjoined table shows the intrinsic brightnesses of various sources.

Table of relative intrinsic brightnesses.

	Candle power per square inch
The sun at zenith through air	1,000,000*
The crater of the ordinary carbon arc	110,000*
Tungsten filament in argon (·65 watts per candle)	7,500†
Oxy-acetylene with pastille	3,000*
Ordinary tungsten filament (in vacuo)	960*
Acetylene flame (burning in air)	36‡
Paraffin flame (enclosed in glass chimney)	10‡

* National Physical Laboratory. † Langmuir. ‡ G. A. Shakespear.

In daylight signalling from aircraft a source of very high intrinsic brightness is absolutely essential, because unlike night signalling it is not merely a question of producing a certain candle power in the beam but it necessitates that a certain minimum of *contrast* shall be afforded when seen against such a comparatively bright background as is afforded by the sky in broad daylight. Unlit, the lamp is a *black* dot in the sky and a certain amount of light energy is used up in (so to speak) obliterating this negative effect. The table makes it at once evident that the electric arc must be employed for really powerful apparatus, but that the tungsten filament incandescent in argon offers far and away the most effective and convenient source

for readily portable signalling outfits. The figure given as originating from Langmuir's investigation of the (so-called) half-watt lamp was communicated by Mr Paterson when he was at the N. P. L.; but the writer's investigations which led up to the design of the Aldis Daylight Signalling Lamp, show that a mean intrinsic brightness of over 17,000 candles per square inch of surface has been attained with the lamp run at an efficiency of ·4 watts per candle. This was only the *mean* intrinsic brightness, however, and the photograph (see Fig. 1) of the incandescent filament shows that different parts of the wire are actually very much brighter than other parts. The illustration is from a photograph of the incandescent filament made by projecting this on to a sensitive plate, so that an accurate, highly enlarged record was obtained both of the apparent size and shape of the filament from the view point of the lens used for projecting the image and also of any variations in brightness in different parts of the filament.

Fig. 1. Incandescent coils shown magnified 8 times.

As there is a great danger, when dealing with the subject of signalling lamps, of introducing ambiguity by referring to the electric incandescent sources as "electric lamps" these latter will always be referred to as "electric bulbs," and in what follows, as a matter of fact, the term electric bulb will always be supposed to mean a high efficiency, so-called half watt (or preferably gas-filled), incandescent electric lamp.

The only source that can compare with an electric bulb for convenience in a self-contained really portable outfit is the acetylene flame, and from the data supplied above it is evident that the intrinsic brightness is only about $\frac{1}{300}$ of that possible in the electric bulb. It would appear, therefore, that for an acetylene projector to attain to the same range as a 5″ diameter electric signalling lamp it would have to be made over 7 *feet in diameter*. This comparison, however, assumes complete flashing in both cases. The electric bulb, therefore, is the best source to employ in a hand signalling lamp provided a complete flash can be afforded.

As regards the optical means employed for projecting the beam it may be remarked that the well known property of a parabola, viz. that rays emanating from the focus are rendered parallel after reflection, has caused this optical device to receive (as will be shown later on) an undue amount of attention from those responsible for the design of such lamps. Again and again inventors appear to hanker after that impossible abstraction a "point source" used in conjunction with a parabola. As a matter of fact, it is at the present time easier to approximate to a point source than it is to approximate to a true point focus for a parabolic reflector suitable for such a source. In other words, with any parabolic reflector commercially obtainable the spreading of the projected beam due to local imperfections of shape is quite as great as that due to the source having appreciable size when the best types of the so-called point sources are utilised.

It will be found convenient to describe the illumination produced in different

parts of the signalling beam in terms of the light patch received on a distant white screen—this being the way in which such beams are practically tested for uniformity and angular size. To investigate the theory of the parabolic projector for affording a complete flash within a certain predetermined angle α it is convenient to consider what each tiny element of the parabolic reflector contributes to the light patch received on a distant screen. It is evident that any such tiny element throws on to the screen a patch whose apparent shape and size is exactly the same as the apparent shape and size of the light source as seen from that portion of the reflector. Its position on the screen is such that the axis of the reflector intersects this light patch at a point corresponding to the apparent position of the focus in the source as viewed from that portion of the reflector. Thus the ideal shape and size of the smallest possible source to fulfil the required conditions is a sphere surrounding the focus as centre whose angular diameter as seen from the forward edge of the parabola is equal to α, and that this forward edge should be in the plane containing the source, i.e. the latus rectum of the parabola should be made equal to the assumed limit of clear aperture imposed for the signal lamp. It is bad practice and wasteful to employ a parabola of shorter focal length than this. It is true that it collects more light energy from the source, but this is scattered over a wider angle in the projected beam. Complete flashing is actually confined to a narrower angle.

Now it is impossible to dispose the filament in an electric bulb so that it shall appear as a continuous incandescent spherical source. Fig. 2 is from a photograph of an incandescent filament forming one of the best approximations to an incandescent sphere which the electric lamp makers were able to produce before the introduction of the drawn wire filament. The photograph was made by projecting the incandescent filament exactly as in the case of Fig. 1. This is known as a "crown pattern" filament, the filament being zig-zagged up and down, and the zig-zags disposed on the surface of a cylinder; in fact the filament is made to follow the shape of the upper edge of the paper crowns so popular with children.

Fig. 2. Incandescent filament shown magnified 8 times.

The best approximation to a spherical source, of course, is obtained by including as many zig-zags as possible disposed on the cylindrical surface and making the height of the zig-zags equal to the diameter of this cylindrical surface. The image of this crown pattern filament, as projected by a good lens in a direction approximately at right angles to the axis of the crown pattern, forms of course an interlaced pattern of lines far removed from anything that could be described as a continuous incandescent sheet. Nevertheless, this pattern filament placed with its

axis coincident with that of a parabolic reflector affords quite a good signalling beam *if the reflector be made large enough*. What happens, of course, is that the reflector is only seen partially flashed everywhere within the angle of the beam, and for night signalling this suffices very well.

The introduction of the tungsten drawn wire filament made the simulation of a continuous surface in electric bulbs possible. Fig. 1 shows the progress made since pre-war days in producing such sources. When it is remembered that the two sources (Figs. 1 and 2) are shown to the same scale of enlargement (eight times natural size) and that the candle power for the gas filled bulb (Fig. 1) was over 100 and that for the vacuum type (Fig. 2) was only 12, it will be seen that the progress in increasing the actual intrinsic brightness of the wire was fully as remarkable as the progress in concentrating the wire so as to simulate a continuous small incandescent sheet. Though the projection of the coil presents the appearance of a continuous incandescent surface, it is very far from being a uniformly bright surface, because the temperature of the wire varies in different parts due to self-heating effects between adjacent rings. Remembering that the actual diameter of the filament is only a few thousandths of an inch, the variations in temperature and brightness of different parts of the surface of the wire are remarkable. The fact also that these periodic variations in brightness from ring to ring are, despite every effort of the designer to obviate them, all to a certain extent reproduced in corresponding variations in intensity in the signalling beam shows what small quantities have to be considered in modern portable daylight signalling apparatus.

Fig. 1 shows that the tungsten drawn wire filament can be wound so as to simulate a continuous incandescent cylinder. It is evident that the best approximation to an incandescent sphere for projecting a circular beam from a parabolic reflector is a close cylindrical coil coaxial with the reflector, (whose outside diameter is the diameter of the equivalent sphere) sufficiently long for the apparent shape as seen from *all useful parts* of the reflector to show no black hole due to looking right through the interior of the coil.

The reference to "all useful parts of the reflector" makes it necessary to call attention to the one great draw-back of the electric bulb used in conjunction with any reflector placed behind it for projecting a signalling beam. The glass containing bulb acts practically as an opaque obstruction in the path of the beam—it is quite impossible in fact to produce a complete flash in such a reflector; the most that can be done is to produce a complete flash in the annulus of the reflector seen surrounding the bulb. Any partial flash seen through the bulb affords always an absolutely negligible contribution to the total signalling beam. It must never be forgotten, therefore, that any reflecting system placed behind the source is to be considered as limited to the annulus enclosed between the outer edge and a central area equal in diameter to that of the glass bulb employed for the electric source; and maximum efficiency will always be attained if a complete flash can be secured in this outer annulus only. In actual practice the inner edge of this annulus subtends so large an angle at the source that it is easy to make an axial coil long enough to be seen as a continuous sheet without any hole in it due to looking right through

the coil from this inner edge. A comparatively short axial coil, therefore, can afford a complete flash in a parabolic mirror annulus.

This obstructive effect of the bulb shows how important it is for the makers of electric bulbs to provide the maximum of candle power *in the minimum size of bulb*. At present for high efficiency signalling lamps intended to be held in the hand the minimum possible size of bulb for a given filament is such that the mirror annulus cannot be brought near enough to the source to afford the necessary angle in the signalling beam. The only thing to do, therefore, is either to introduce such deformations in the parabolic mirror as will spread the beam by the necessary angle or to wind the incandescent filament as a larger and more open spiral coil. As a matter of fact, it is best to do a little of both, but this, of course, means adopting partial flashing for the whole of the signalling beam and using a lamp of larger diameter than is theoretically necessary. Thus the makers of electric bulbs have to a certain extent taken away with one hand from the designers of signalling lamps what they have given with the other. They have afforded small sources of exceptional intrinsic brightness, but put them in such large bulbs that they cannot be used to full advantage.

The most obvious way to get over this obstructive effect of the bulb is to abandon the idea of a mirror placed behind the bulb and adopt a dioptric system in front of it. Here again, however, the radius of the bulb imposes a definite limit to the possible proximity of the optical system and as a matter of fact the expense and complexity, not to mention the weight, of a system of dioptric and catadioptric rings placed in front of an electric bulb renders this system impracticable. Where a fairly wide angle is demanded in the signalling beam the designer of commercially practicable signalling lamps is forced to adopt partial flashing in a mirror placed behind the electric bulb.

Now the commercially procurable small parabolic reflectors suffer from two grave defects, first, the uncertainty of their actual shape*, and, secondly, their tendency to tarnish and so to lose rapidly in reflective power in use. A much better reflector for signalling purposes and more aplanatic too, is afforded by the Mangin mirror or lens mirror. Fig. 3 shows correctly to scale a principal section of the mirror used in the daylight signalling lamp designed by the writer. The theory of flashing in such a mirror can, as a matter of fact, be best dealt with in reference to what may be termed the focal caustic for the view point. All the possible directions in which the eye of a distant observer on the mirror axis can receive light from the mirror annulus form a system of lines all parallel to its axis. These directions can be traced optically through the lens mirror as shown in Fig. 3, and after reflection and refraction they determine in any principal plane the focal caustic surface corresponding to a beam of perfectly parallel light received on the mirror parallel to the axis. This focal caustic can be determined accurately for any given lens mirror by exact computation, and it has definite size, shape, and what may be termed colour thickness, because on *refraction* the ray directions are split up and the final

* Not so much, be it noted, the inaccuracy of their shape but the element of pure chance as to what the departures from parabolic shape are.

caustic has a red inside limit and a blue outside limit for visible light. It must not be forgotten that the central part of the mirror is obstructed by the bulb and, therefore, the only part of the focal caustic to be taken account of is that formed by ray directions incident on the useful mirror annulus, and this limitation will apply in all that follows.

In any well-designed mirror annulus the size of the focal caustic is very small

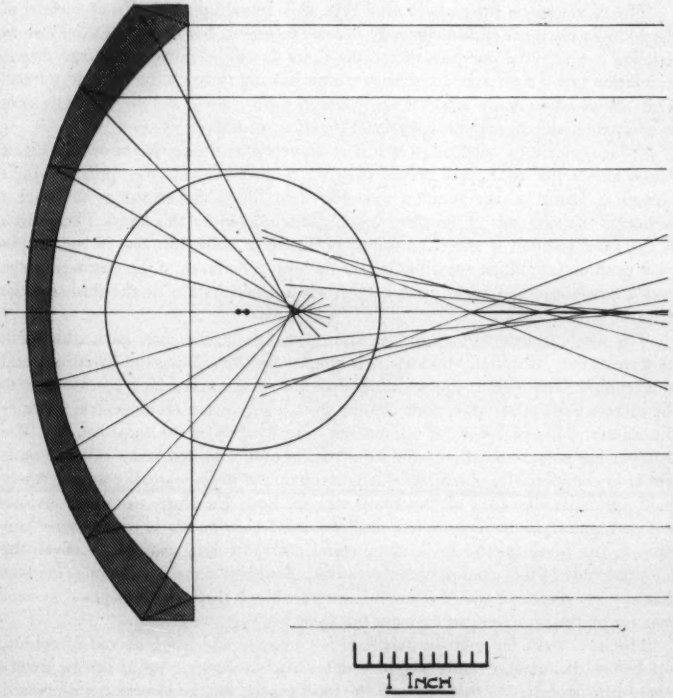


Fig. 3. Section of Aldis Signalling Mirror and its axial focal caustic, shown actual size.

in relation to the size of mirror. Unfortunately, if designed to produce minimum size, there is an inevitable tendency to increase colour thickness, which imposes a limit on what is wise in this direction. Still the fact remains that the focal caustic is small in relation to the mirror, and is also generally much smaller than the actual focal caustic afforded by any commercially obtainable parabolic reflector. It is,

however, not small in relation to any really useful electric bulb source for projecting a beam of reasonable angular size. The circle in Fig. 3 shows correctly to scale the diameter of the glass containing bulb for the electric source, not, however, in its focussed position but displaced towards the mirror. This makes plain what a comparatively small annulus is left unobstructed. The two black dots shown near the centre of the bulb indicate the actual outer diameters (·033 inch) and the relative location of the two incandescent coils.

The focal caustic for parallel axial rays after refraction at the front surface of the Mangin mirror is shown correctly to scale in Fig. 3, but displaced axially so as to bring it within the compass of the diagram. It will be noted how large this is in relation to the final axial focal caustic after passing through the Mangin mirror. This shows how greatly superior the Mangin mirror is for flashing small sources as compared with any simple spherical refracting or dioptric system.

What is meant by small sources is best conveyed by the section shown in Fig. 4 which shows the axial focal caustic (for red colour only) for the Aldis Daylight Signalling Lamp mirror annulus, enlarged eight times, and in the same figure a section of the two coils of the electric bulb standardized for this lamp. The section at the axial position is shown, not only correctly to scale, but also in the proper focal position (as will be shown later) for flashing the mirror to the best advantage over a predetermined angle, with a fairly abrupt occultation of the flash on the margins of the beam.

The whole caustic surface divides up into two parts, the outer part, which will be termed the "trumpet," because it is commonly shaped like the flared out end of a trumpet, and a short line forming a portion of the axis which corresponds to the intersections of ray directions coming from a succession of concentric zones of the mirror. This part will be termed the "axial part" of the focal caustic. The trumpet has colour thickness—the axial part is only coloured at its extreme ends and is in commercially obtainable Mangin mirrors of average quality an extremely good approximation to a mathematical straight line. Evidently the most obvious and easy way to insure a complete flash for axial view points is to surround this part of the focal caustic by a close spiral coil fully long enough to cover the coloured ends of this axial part of the caustic. If this is done it is at once evident that any ray direction traced from the observer's eye through the optical system ends up on the incandescent filament for every visible wave length.

The axial flash for such an axial coil is complete and quite devoid of colour, but before the angular limits within which complete flashing holds can be determined it is necessary to find out how the focal caustic for the view point alters and moves in relation to the source as the view point is moved off the axis. Now there is a point which may be termed the optical centre on the axis of the Mangin mirror, about the same distance in front of the focal caustic as the mirror is behind it, which is such that ray directions passing through it after reflection at the mirror return along themselves. If a fixed distant view point be taken, and if this optical centre be regarded as fixed, and the line joining the view point to the optical centre be termed the "axis of vision," then the ray directions all drawn from the view

point parallel to the axis of vision determine, after being traced through the Mangin mirror, a focal caustic which, as a matter of fact, is remarkably constant in shape, size, position, and colour thickness as the Mangin mirror is rotated through any *small* angle about the fixed optical centre. The chief deformations, of course, occur at the larger end of the trumpet, but they are not important.

It should be noted that this is a property of the Mangin mirror annulus not

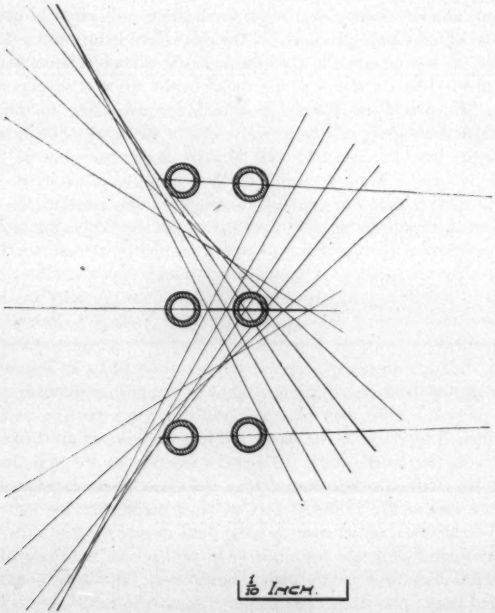


Fig. 4. Axial focal caustic of Aldis Signalling Mirror, shown magnified 8 times.

shared by other less truly aplanatic optical devices such as a parabolic reflector whose focal caustic is a point for the axis, but something very far removed from a point the moment the axis is departed from. It is, of course, only roughly true of the Mangin mirror but quite a good rough first approximation when such comparatively small angles are dealt with as actually occur in daylight signalling lamps. It follows from this property of the Mangin mirror that the effect of imperfect aim may be described as equivalent to swinging the incandescent source about the optical centre through a small angle equal to the angular error of aim in relation to a fixed and definitely shaped focal caustic, viz. the one computed for

parallel axial ray directions. The use of this focal caustic in conjunction with the computed optical centre for the mirror is now made apparent. To be any good the source should have dimensions comparable with those of the focal caustic and quite large in comparison with colour thickness; it must be situated so as to include as large a part of the focal caustic as possible; and when rotated about the optical centre it must continue to include a considerable portion of the focal caustic, and should preferably cease contact with the trumpet and all the cone of rays enveloping the same abruptly and simultaneously. Since small angles only are to be considered, the nature of the illumination produced off the axis of the lamp can be derived by considering how the source cuts the axial focal caustic when it is laterally displaced perpendicular to the axis a distance proportional to the angle of divergence under consideration. The two displaced coil positions corresponding to the extreme limits of the beam are shown in Fig. 4, and indicate an adjustment in which the angular size of the halo produced by the forward coil is the same as the outer angular size of the *annular* beam projected by the coil nearer the mirror.

The point to note is that the light patch projected has angular size as much due to the spherical aberration of the mirror as to the source having size. It has already been pointed out that the real problem presented by the electric signalling lamp is that when the filament coil is wound close enough to present the appearance of a continuous incandescent cylinder the size of the bulb prevents any reflecting optical annulus placed behind it being brought close enough to project a beam of useful angular width. Thus the designer is compelled to adopt a relatively large mirror annulus, and this means that spherical aberration plays an important part in determining the size and distribution of light in the projected beam.

Chromatic aberration also may play a fatally important part in reducing the equivalent intrinsic brightness of the flash, and it is very important to keep down colour thickness in the focal caustic to a mere fraction of the coil diameter if efficiency is to be attained. The way colour thickness goes up when spherical aberration (or the size of the trumpet) goes down is made clear by Fig. 5, which shows enlarged eight times actual size the axial focal caustic for the Aldis Daylight Signalling Lamp mirror, and that for a mirror of similar size but designed to give minimum spherical aberration *for the useful annulus only*. It will be noted that the trumpet is folded back upon itself. The colour thickness in both cases is indicated by shading. The section of the incandescent coils as used in the Aldis Lamp, shown correctly to scale on the left, makes it at once evident that the focal concentration has been carried too far for such a source, considering the colour thickness introduced.

To consider specific cases in more detail, the axial coil method of obtaining a complete flash from a mirror annulus when considered in the light of the foregoing shows that only a comparatively narrow beam can be afforded by such means, though this will be greatly improved if the diameter of the coil be made more than large enough to bridge the gap between the axial part of the caustic and the small end of the trumpet. For wide beams, however, and especially for transmitting beams affording a rectangular or oval light patch it is best to use coils placed at right angles

to the axis of the mirror. If one coil be placed with its axis in the plane of the circle of least confusion of the focal caustic it will project a good rectangular-shaped light patch showing fairly even illumination and surrounded by no halo, because the coil ceases contact with the focal caustic and the extreme cone of light rays enveloping the caustic simultaneously. If this coil is moved away from the mirror the bright portion of the light patch contracts and a large and weak halo surrounding it is

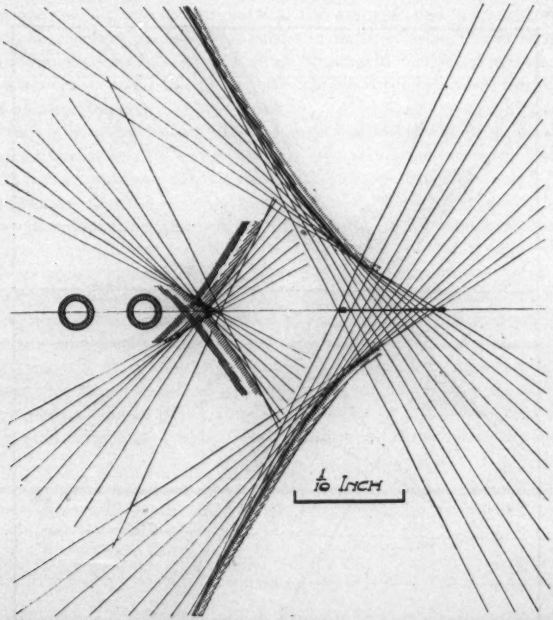


Fig. 5. Axial focal caustics of different 4 inch diameter Mangin Mirrors, shown magnified 8 times.

seen rapidly growing in size. As it is moved inwards towards the mirror the light patch grows rapidly in size, its edges become brighter and fringed with colour, and ultimately it shows a dark centre. All these effects can be deduced by a careful consideration of the focal caustic, noting how in the various axial positions the coil approaches and leaves the caustic surface as it is moved laterally across the system of rays, and not forgetting the effect of colour thickness.

The adjustment aimed at in the Aldis Daylight Signalling Lamp was to use two coils, the one nearer the mirror located so as to project a light patch having a dark rectangular centre with a bright ring surrounding it, the other located so as

to project a light patch having a bright rectangular centre *coinciding with the dark centre of the former beam* surrounded by a halo *of just the same size* as the outer ring in the former beam. Obviously this means a very nice adjustment, but the interesting point to note is that, given the focal caustic and the optical centre, the size and disposition of the two coils can be laid down on paper to fulfil this condition, and the angular limits of the beam predicted with tolerable accuracy.

Fig. 4 shows in addition to the axial position of the incandescent coils the

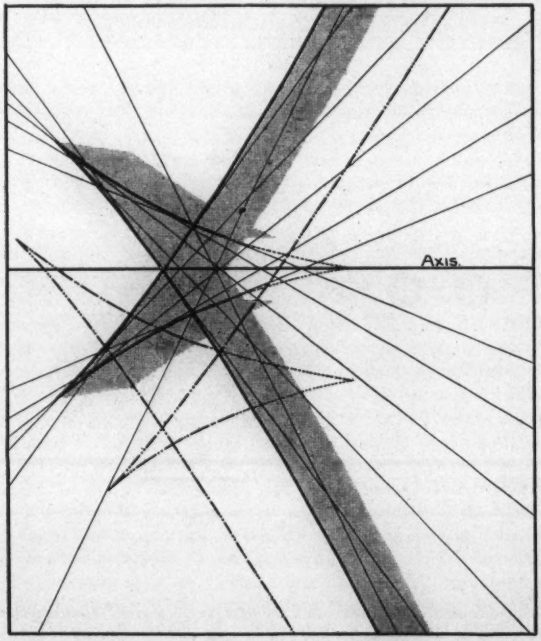


Fig. 6. Focal caustics of 4 inch diameter Mangin Mirror, shown magnified 32 times.

position displaced off the axis by an amount which corresponds to the extreme limits of the projected beam on the assumption, however, that the focal caustic remains unchanged for such comparatively large departures from the axial view point. Making this assumption, it would be asserted that, provided only the light passing forwards from the naked source were cut off by a shield placed in front of it, Fig. 4 would determine the theoretical angular limit outside which no light would be received by any observer.

As a matter of fact, there are two very important practical reasons causing light to be projected outside the margins of the beam, both attributable to the glass containing bulb surrounding the source. This is approximately spherical in shape and is generally of variable thickness, being thinnest at the drawn off pip end. This has two effects; the first by reflection causes an inverted image to occur generally near to, but unfortunately seldom in, the plane of the coils forming the original source. In the second place the computed focal caustic for any lens mirror differs from the one actually effective in practical use owing to deformations introduced by refractive effects in passing through the containing bulb. The former defects result in the projection of a separate "ghost" beam which militates against secrecy, and the latter defect spoils the shape and evenness of the beam and throws out an irregular halo surrounding the light patch.

The effects of ghost images of the source due to reflections at the front or refracting surface of the Mangin mirror can, to a very large extent be obviated by placing an annular stop at a suitable distance in front of the source, and it has been found in practice that the really serious sources of lack of secrecy have been due to the two above-mentioned causes, both attributable to the glass containing bulb. It has already been pointed out that, provided a certain amount of colour thickness in the focal caustic can be tolerated, the spherical aberration can be reduced to a remarkable extent and in this case the part of the focal caustic that has been termed the trumpet is so to speak folded back upon itself. Fig. 6 shows, enlarged 32 times, the axial focal caustic and the rays enveloping it for a mirror annulus carefully designed to afford large angular aperture with minimum spherical aberration. This mirror annulus has practically the same clear aperture as the mirror shown correctly to size in Fig. 3 and hence this caustic is shown at four times the enlargement used for Figs. 4 and 5. The specification for this mirror annulus is

Radius of first refracting surface	$2\frac{1}{32}''$
,, ,, second reflecting surface	$3\frac{1}{64}''$
Thickness at centre	$\frac{1}{10}''$
Clear aperture of front surface	$4''$
Diameter of central part cut out by the glass containing bulb	$2''$
Refractive index of glass for red ray	1·533
,, ,, ,, blue ray	1·544

The six pairs of rays actually drawn correspond to ray directions intersecting a plane containing the extreme front edge of the mirror at successive $\frac{1}{4}''$ intervals measured from the axis and starting at $\frac{3}{4}''$ distance from the axis, which includes slightly more than the useful annulus. The shaded contour indicates the colour thickness of the focal caustic which is dangerously large for such a small source as was standardised for the Aldis Daylight Signalling Lamp. In addition, of course, this focal caustic indicates that the angle afforded in the beam by this source used in conjunction with this mirror would not be large enough.

An interesting point in connection with Fig. 5 is that the second non-axial focal caustic (shown by a chain-dotted line) corresponds to accurately computed ray directions after passing through the mirror annulus initially incident upon

it from a distant view point ¾ of a degree off the axis. It will be seen that they determine a focal caustic very similar in shape and size to the axial focal caustic, though displaced from it by an amount considerably greater than its circle of least confusion. The colour thickness of this displaced caustic is not shown for the sake of clearness.

Considering the labour of computing these non-axial caustics, it is evident that for small angles the displaced axial caustic may be taken as a very good guide as to where the ray directions come for a non-axial view point. Thus a study of the complete axial caustic in relation to the optical centre of the mirror affords a very good means of judging the best type of source to employ and the best focal position for the projection of a beam of given intensity free from colour within predetermined small angular limits.

DISCUSSION

Professor Cheshire said that when he was at the Ministry of Munitions he was particularly struck by the way in which Messrs Aldis Bros. had tackled a number of pressing difficulties during the war, and he was sure that peace problems would be dealt with by them in an equally efficient manner. He thought that the lecture set out very clearly an interesting optical problem and its solution. The ray diagrams which Mr Aldis had exhibited would prove to be of great use to future designers.

Major Henrici testified that in the Lucas modification of the Aldis lamp the bulb is automatically fixed in its correct position, and that the direction of the beam of light did agree with the sight.

Mr R. S. Whipple said that it was evident from the lecture what a large amount of experimental work had been necessary to evolve what appeared to be a simple optical instrument. He would like to know whether electric light bulbs similar to those Mr Aldis had used in his signalling lamps would be available for general experimental purposes, such as galvanometer work. He asked if Mr Aldis knew whether the inert gas in the bulbs was under pressure.

The author, in reply, said he was of the opinion that the "focus" bulbs (as he called them) had a distinct future before them. He thought that Messrs Siemens must have a large number of bulbs for disposal.

The bulbs were filled with nitrogen (or, preferably, argon) at ordinary pressures. They were 14 volt bulbs which consumed ·4 watt per candle, and had a total candle power of about 110 candles.

THE UNAIDED EYE

PART III*

BY JAMES WEIR FRENCH, B.Sc., F.O.S.

MS. received, 31st January, 1920. Read and discussed, 15th April, 1920

SYNOPSIS

Coincidence observations; description of apparatus and safeguards. Coincidence accuracy observations with results. Variables that affect the observations. Distinction between precision and adjustment errors. Variation of precision with length of thick coincidence lines, similar results for thin lines. Minimum precision error relatively to length of coincidence line. Effect on precision of separating line thickness. Adjustment errors. Effect on adjustment of separating line thickness. Variation with length of line. Variation with thickness of line. Effect of premature judgment. Verticalising power of eye. Relation to coincidence adjustment error. Effect of astigmatism on error.

Theories of vision. General review of subject. Statement of proposed series theory. The location of images independent of cone diameters. The form apparatus. The colour apparatus. Its explanation of red, green and blue colour blindness, monochromatic threshold vision. Anomalous trichromatic vision. Polychromatic foveal vision. Contrast phenomena. Suggested functions of retinal layers of ganglion cells. Their relation to contrast and irradiation phenomena. Axis cylinder of nerve fibres.

WHENEVER an accurate measurement is desired, it is well known that the best results are obtained by the use of coincidence methods when such methods can be employed. For the accurate reading of a graduated circle a vernier is used, and the observation then depends upon the discrimination between the continuity or discontinuity of a line on the circle and one on the vernier. The application of coincidence methods of observation to range-finders is already well known, and as the application of such methods is becoming more general, as for example, in the reading of the bubble of surveying instruments, the particulars of certain systematic experiments recently carried out by the writer on the coincidence or aligning power of the unaided eye may be of interest to the members.

As it is essential in precision experiments that the genuineness of the observations should be unquestionable, the arrangement of the coincidence apparatus employed will first be described. However honest the observer is, his readings may be unconsciously influenced by circumstances such as the knowledge that in the previous observation the operating handle occupied a particular position.

Figs. 1 and 2 show the coincidence apparatus employed, the former being a front view and the latter a plan. *A* and *B* are two metal plates capable of relative movement along a well-fitted line of separation *CD*. Lines *E* and *F* were engraved, painted or mounted as the case might be upon the matt surfaces of the plates. Black

* Parts I and II appeared in *Trans. Opt. Soc.* 20 (1919), 209 and 21 (1919), 1 respectively.

lines upon a white ground were most commonly used, but white lines on black grounds and contrast colours were also tested. Whereas plate *A* was fixed, plate *B* could be translated horizontally by means of an electrically operated step by step motor *G* acting through the intermediary of a pinion *H* and wheel *J* and a micrometer screw *K* working in a nut *L* integral with the translatable plate *B*.

The gearing was such that one step of the motor corresponded with a displacement of the plate of one ten-thousandth of an inch, which represents an angular displacement of one-tenth of a second subtended at a distance of 200 inches.

From the motor *G* a flexible cable was carried to a transmitter at the observer's station, the distance of which was varied from 8 to 115 feet. By rotating the transmitter handle forwards or backwards the plate *B* could be translated step by step towards the left or the right as required at any speed up to 20 steps per second.

For the determination of the displacement of the plate *A* relatively to *B* there was provided a microscope *M* mounted rigidly upon *A*, and so arranged as to

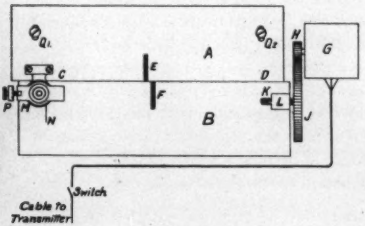


Fig. 1. Front view.

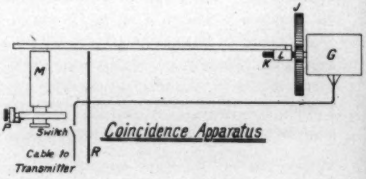


Fig. 2. Plan.

observe an index line *N* engraved upon *B*. The position of the line was measured by means of a micrometer eyepiece *P*.

Adjustment of the width of the separating line *CD* was effected by a vertical movement of the plate *A* which was clamped in position by the screws Q_1, Q_2, the necessary slotted holes being provided in the plate.

Now to prevent unconscious manipulation of the observations there was provided in the electrical connections between the motor *G* and the transmitter a switch controlled by the separate microscope reader, who could interrupt the circuit at frequent intervals during the operation of making the coincidence observations. The movement of the switch could not be seen by the coincidence observer, and the microscope scale reader could not see the coincidence objects which were shielded by a diaphragm *R*. After each setting of coincidence a signal was transmitted to the microscope scale reader, who then noted the indication.

In the first instance a series of observations was made with the particular object of obtaining some idea of the greatest precision that could be realised, and for this purpose no limit was imposed as regards the time taken in making the several observations. An error of observation may be divided into two components—one

the precision error which indicates the average accuracy with which an observation can be repeated irrespective of its value, and the other the adjustment error, which represents the deviation of the mean observation from the true known value. The method of reckoning these two types of errors has already been described in the second lecture.

Black lines having a width of 0·43 inch mounted on a white background were used and the separating line was made as fine as possible, its thickness being about four-thousandths of an inch. Nine sets of observations lettered A to I, each comprising five readings, are included in Table I. Set A is given in full, but columns (3) and (4) alone are indicated in the subsequent sets. For the first three sets A, B and C, one revolution of the micrometer head equalled 0·0023 inch. The distance of the observer was 112 inches, therefore one revolution of the micrometer head represents a subtended angle of 4·32 seconds. For the sets D, E, F, G, H and I a different microscope was used, one revolution of the micrometer head of which represented a displacement of 0·0056 inch. The distance was 488 inches and therefore one revolution of the head represents a subtended angle of 2·36 seconds.

Table I

In sets A, B and C one div. = 4·32 secs.

A				B		C	
(1)	(2)	(3)	(4)	(3)	(4)	(3)	(4)
Index reading	Actual reading	(2)—(1)	Diff. from mean				
17·18 revs. of micrometer head	18·44	1·26	0·06	1·22	0·43	0·75	0·29
	·44	1·26	0·06	0·58	0·21	0·95	0·09
	·54	1·36	0·16	0·76	0·03	1·05	0·01
	·35	1·17	0·03	0·70	0·09	1·13	0·09
	·12	0·94	0·26	0·68	0·11	1·30	0·26
	Mean	1·20	0·114	0·79	0·17	1·04	0·15
Equivalent seconds 0·49					0·74		0·65

In sets D, E, F, G, H and I one div. = 2·36 secs.

D		E		F	
(3)	(4)	(3)	(4)	(3)	(4)
1·58	0·16	1·46	0·26	1·90	0·27
1·27	0·15	1·57	0·15	1·75	0·12
1·12	0·30	1·97	0·25	1·76	0·13
1·79	0·37	1·67	0·05	1·20	0·43
1·32	0·10	1·95	0·23	1·53	0·10
Mean 1·42	0·21	1·72	0·19	1·63	0·21
Equivalent seconds 0·5			0·45		0·5

Table I (*continued*)

G		H		I	
(3)	(4)	(3)	(4)	(3)	(4)
1·60	0·02	2·13	0·22	2·28	0·01
1·45	0·17	2·46	0·11	2·42	0·15
1·33	0·29	2·70	0·35	2·72	0·45
1·42	0·20	2·20	0·15	2·05	0·22
2·30	0·68	2·25	0·10	1·87	0·40
Mean 1·62	0·27	2·35	0·19	2·27	0·25
Equivalent seconds 0·64			0·45		0·59

Table II

Extreme difference of readings in each set of five

A	B	C	D	E
1·36–0·94	1·22–0·58	1·30–0·75	1·79–1·12	1·97–1·46
Divs. 0·42	0·64	0·55	0·67	0·51
Secs. 1·82	2·77	2·38	1·58	1·20

F	G	H	I
1·90–1·20	2·30–1·33	2·70–2·13	2·72–1·87
Divs. 0·70	0·97	0·57	0·85
Secs. 1·65	2·29	1·35	2·00

As already stated it is only the precision that is of interest at the present moment. The observer was not concerned with the absolute value of the readings, but only with the precision with which the readings could be repeated. In the first series A, B, and C the respective precisions for sets of five observations were 0·49, 0·74, and 0·65 seconds of angle at the unaided eye, the average thus being 0·63 seconds. From Table II it will be seen that the extreme variations of the readings of each set were 1·82, 2·77 and 2·38 seconds, the average being 2·32 seconds.

In the second series of readings D to I, taken under different circumstances, the respective values of the precision are 0·5, 0·45, 0·5, 0·64, 0·45, and 0·59 secs. and the average is 0·52 seconds. The extreme variations of the readings in each of the respective sets are 1·58, 1·2, 1·65, 2·29, 1·35, and 2·0, the mean being 1·68 seconds. The average precision for the nine sets is 0·58 seconds.

In the foregoing precision tests, the only purpose of which was to attain the greatest possible precision, light from the object alone reached the observer's eye, all extraneous light being excluded. Throughout subsequent tests extraneous light was only excluded by means of window blinds and the conditions were therefore more nearly normal.

In all cases the coincidence observer had no knowledge of the readings or any information as to whether his performance was good or bad. It will be understood

that in the subsequent tests about to be described the main object in view was to determine the comparative effect of a variety of conditions upon coincidence. Of these conditions the principal ones that were varied in the tests were as follows, but owing to limits of space only a selection of the results will be included in this lecture:

(1) The width of the coincidence line.

(2) The height above or below the separating line of the coincidence line.

(3) The width of the separating line.

(4) The distance of the observer.

(5) The colour contrast, such as black or white.

(6) Verticality of the coincidence lines. A small sidewise inclination may seriously affect the results.

(7) Illumination.

(8) Effect of horizontal, vertical, and oblique reference lines.

(9) Position of the observer's head. A sidewise inclination of the head may affect the results.

Precision observations as distinct from adjustment error observations will first be considered.

Fig. 3 represents a typical series of results obtained by an alteration of the height of the coincidence line measured from the separating line, and therefore of the angle subtended by the coincidence line height at the observer's eye situated at a distance of 200 inches from the object. Similar curves are shown corresponding with five different widths of separating lines, subtending angles of 4, 82, 185, 526, and 1130 seconds respectively. Ordinates represent angular heights expressed in minutes and abscissae the precision in seconds of angle. The thickness of the coincidence line was 0·43 inch.

A similar series for a much finer coincidence line having a thickness of only 0·05 inch is indicated in Fig. 4, the distance of the observer being again 200 inches. As before each curve corresponds with a particular thickness of separating line, the thicknesses in the two series being approximately though not exactly the same.

Reverting to Fig. 3 it will be seen that in all the curves, as the height of the coincidence line is diminished, the precision error increases, until a point is reached where the height subtends an angle of about 0·6 minute, under which conditions the eye appears incapable of observing with precision any want of alignment. In the case, however, of the very broad separating line, subtending 1130 secs., the limit appears to be reached at an angular height of about 2 to 3 minutes, and a similar but smaller tendency is observable in the 526 seconds curve.

That there should be greater difficulty in aligning the broad partial images the thicker the separating line, is to be expected, but this is not confirmed in the second series, Fig. 4, where it should be observed that the coincidence line was much thinner, being 0·05 inch as compared with 0·43 inch. It will be noticed that in the latter series the lower limit at which the resolution fails is more constant than in the case of the thick coincidence line.

As the height of the coincidence line increases the precision error decreases,

but for each width of separating line there is a limit beyond which an increase in the height has no further influence on the accuracy. Thus from the curves of Fig. 3 it will be seen that in the case of the thickest separating line, namely 1130 secs., the precision error decreased as the height of the coincidence line was increased until at an angular height of about 12 minutes a limiting precision of 5·1 seconds was reached. An increase of the height had thereafter practically no effect. It would appear that, in estimating the alignment, so far as precision is concerned, the eye takes no account of the portions of the coincidence line beyond this limiting angular height. In the case of the thinnest separating line, namely 4 secs., the limiting precision is about 1·1 seconds at an angular height of about 9 minutes.

The curves *F* of Figs. 3 and 4 indicate how the minimum error of precision varies with the angular width of separating line. The thinner the separating line

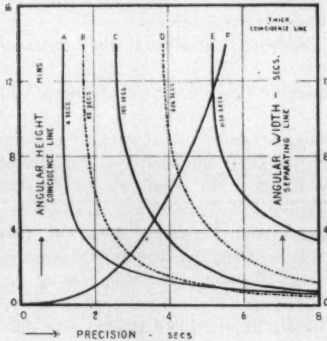


Fig. 3.

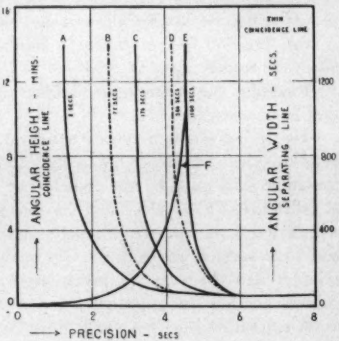


Fig. 4.

the greater the precision attainable, and to a slight extent, the smaller the height of the coincidence line necessary for the attainment of this limiting precision.

Owing to the limits of space it will not be possible to give particulars of coincidence results taken under the various conditions. Fig. 5, however, may be referred to briefly as indicating how the precision of coincidence observations is directly affected by the thickness of the separating line.

In the particular example illustrated the distance of the observer was 200 inches and the thickness of the coincidence line was 0·43 inch, which thus subtended an angle of 7 minutes. Three curves corresponding with coincidence lines of three different angular heights, namely 15·8 minutes, 4·28 minutes and 0·85 minutes respectively, are indicated. As is to be expected from the previous diagram, the very short line is widely separated from the longer lines subtending more than 4 minutes. Ordinates represent the thickness of the separating line expressed angularly in seconds, and abscissae the precision of coincidence setting. The curves in question have been smoothed and only indicate the general tendency. As the

thickness of the separating line increased the precision error increased, the increase being comparatively regular in the case of the very short coincidence line. For the longer lines the precision error tends towards a maximum value in each case, the maximum for the longer line being about 6 seconds, and for the intermediate line about 9 seconds, but too great importance should not be placed upon these particular results as consistent readings could only be obtained with difficulty when the separating thickness was great.

From the practical point of view the adjustment errors are possibly of more interest than the precision errors where absolute measurements are concerned. There is more prospect and necessity for an improvement in the comparatively large adjustment errors of coincidence than in the precision errors.

As the width of the separating line is increased the adjustment error increases approximately in direct proportion, but the rate of increase is very variable, not only for different lengths and thicknesses of the coincidence line, but also for

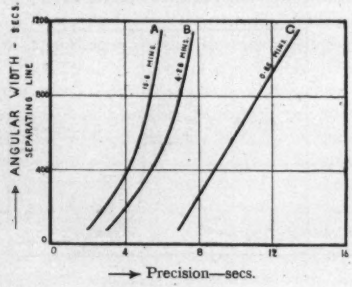


Fig. 5.

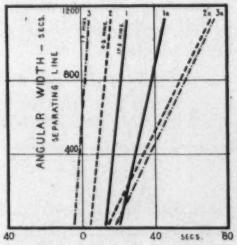


Fig. 6.

different conditions and observing eyes. This is illustrated in Fig. 6. Ordinates represent the angular thickness of the separating line which was varied from a few seconds up to about 1100 seconds. Abscissae represent the adjustment errors in seconds of angle. Those on the right were obtained by moving the lower image into coincidence from the right and similarly those on the left by movement of the lower from the left. It will be observed that the curves of each pair are not disposed symmetrically on the left and right of the zero ordinate. For this particular eye the curves which normally should lie on the left-hand side lie almost wholly on the right. There is an apparent displacement of the mean zero of about 20 seconds for the finest separating line. As the thickness of the separating line is increased the divergence and displacement appear in general to increase, but the results obtained by different eyes vary greatly, the primary cause being probably visual abnormalities. The change in the slope of the mean zero line of each pair of curves for the coincidence lines of different heights was characteristic of other sets of observations. That is, as the length of the line was diminished, the mean zero line

became more oblique. The difference in the slopes of the curves of each pair should also be remarked.

The variation of the adjustment error with the angular height of the coincidence line is indicated in Fig. 7, where the abscissae are as in the previous diagram, and the ordinates represent angular heights, in minutes, of the coincidence lines. The curves of each pair are separated as the result possibly of premature judgment, and when the height is very small the divergence increases rapidly and may become very great. Three pairs of curves for three different thicknesses of separating line, namely about 1 second, 526 seconds and 1130 seconds respectively, are indicated. All three pairs are displaced relatively to the zero line and towards the right side. The curves in question are for a particular eye. Curves similar in form but having entirely different values particularly as regards their displacement may be obtained for the separate eyes of one observer.

There is a natural tendency to be prematurely satisfied with the setting of the images, and as any attempt to compensate this tendency was avoided a difference

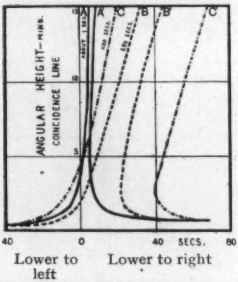


Fig. 7.

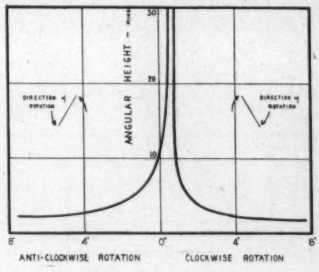


Fig. 8.

between the two sets of readings obtained by moving the image into coincidence from one side and the other is to be expected. From the diagram it will be seen that the interval between the right and left curves is comparatively great when the separating line is thick, and that the interval is only about $\pm$ 2 seconds when the separating line thickness is less than a second. Under the conditions of these particular observations illustrated in Fig. 7, the adjustment error in the case of the very fine separating line, curves A and A', is practically independent of the height of the coincidence line except when the height is less than about 3 minutes. That is to say the curves, except at the lower end, are parallel to the vertical zero line from which, however, they are displaced by a small amount towards the right. In the case of the thicker separating lines the curves above the 3 minute position are parallel to an oblique mean line. The displacement of the mean lines from the zero and the separations of the curves of each pair increase with the thickness of the separating line.

Similar characteristic curves have been obtained for white separating lines with

white coincidence lines on a black background, instead of black lines on a white ground, such as were used in the experiments described, and also for single edges instead of lines. The results differ principally in degree, being in general the more accurate the finer the lines.

From the results of other experiments it was concluded that the displacement of the mean adjustment line was related in some way to the verticalising power of the eye. It is well known that a line which appears vertical to the one eye may appear inclined sideways to the other eye, and that the verticalising power of different eyes varies considerably, from 0 to about 2 or 3 degrees in either the clockwise or counter-clockwise direction. That the amount and character of the adjustment error were very variable was clearly demonstrated by a comparison of the observations of a large number of persons.

To obtain more precise information regarding the verticalising power of the eye, the observer was placed at one end of a black chamber having at its opposite end an externally illuminated line, the orientation of which could be recorded by a separate operator. As there were no reference marks whatever that would aid the observer in the setting of the line, the verticalising sense could only be attributed to the action of gravity on the body of the observer who was seated with the feet not resting on the ground, or to the action of gravity on the contents of the semi-circular canals of the ears or on the eye and its contents. Fig. 8 shows the results of a series of verticalising experiments made upon lines of various lengths. Ordinates represent the angular heights of the various lines expressed in minutes of subtended angle, and abscissae to the right and left of the zero centre line represent angles of rotation of the line in a clockwise or anti-clockwise direction.

There is a remarkably close resemblance between these verticalising curves and the coincidence adjustment curves previously illustrated, particularly as regards the displacement of the mean ordinate. As the length of the line is diminished the estimation of the vertical is not appreciably affected until the height subtends about 5 to 10 minutes, when the verticalising errors rapidly increase. In the case of the coincidence adjustment experiments there was the same rapid increase of the error when the height of the coincidence line was reduced below 5 minutes. It will be seen that this particular observer, whose eyesight was reported by an oculist to be very good, was able to verticalise a line to within about 40 minutes in the case of the longer lines, and it would be interesting to know to what sense this verticalising power of the eye is attributable.

Curve *A* of Fig. 9 shows the precision with which the setting of the vertical could be repeated. The minimum precision error of about 10 minutes is reached in this instance when the coincidence line has an angular height of 20 minutes or more. Below 20 minutes the error rapidly increases. The curve *B* represents the same observations plotted logarithmically. Curve *A* can therefore be represented by the equation

$$\text{precision error} = \left(\frac{316}{\text{angular height of coincidence line}}\right)^{0.9}$$

where the precision and height are expressed in minutes of angle.

It was concluded that the displacement of the zero was due to some defect of the particular eye, and that the defect in question was one of astigmatism. This conclusion was tested and confirmed by placing in front of the observer's eye astigmatic lenses which could be rotated by definite amounts. It was found that the mean adjustment line in the verticalising and coincidence experiments could be shifted from one side of the zero line to the other. Fig. 10 shows the effect in coincidence adjustment experiments of the rotation of a 0·75 dioptre astigmatic lens placed immediately before the observer's eye. Ordinates indicate the angular position of the axis of the cylindrical lens, and abscissae the position of the mean adjustment line. The desirability of correcting the defects of the eye when making precise absolute observations was already well known to the writer, more particularly in range-finding observations, but it should be remembered that such correction must be made with the greatest care and judgment, as otherwise the errors, instead of being improved, may easily be increased.

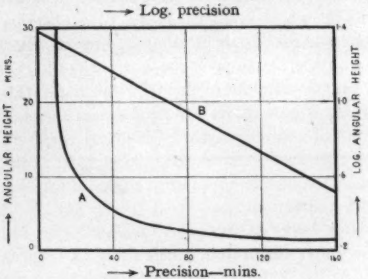


Fig. 9.

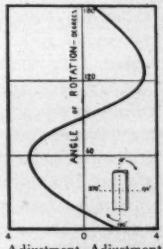


Fig. 10.

As I wish to consider certain theoretical aspects of the mechanism of vision it will not be possible on this occasion to discuss the variations of precision and adjustment under other conditions than those already mentioned, such as, for example, the variations dependent upon the distance of the observer, or the effect of oblique lines of reference, or to deal with the power of the eye in the discrimination of objects of various forms.

It is hardly possible to make measurements of the performance of the eye without being tempted to imagine a mechanism that will explain them. If the existing theories satisfactorily explained the great variety of phenomena that have been recorded by various workers, to add to the number would not be justified. But the very fact that so many theories have been propounded is in itself evidence that no one theory is at present wholly acceptable.

It is unnecessary to refer in detail to these theories which have all been the subject of much controversy; a brief summary must suffice. First there is the pioneer trichromatic theory of Dr Thomas Young, who presupposed the existence of three

types of retinal receptors capable of appreciating red, green, and violet light vibrations. Each receptor apparently responded solely to a particular vibration, and difficulty accordingly was experienced in explaining certain colour mixture phenomena. Helmholtz to some extent overcame these difficulties by assuming that all three receptors responded to each of the three fundamental vibrations, but in varying amounts. Thus red vibrations according to their intensity affect the red receptors to the full extent, and the green and violet only slightly. Clerk Maxwell, Fick, and König substituted blue or indigo blue for violet as one of the fundamental colours, and the last mentioned worker derived from his experimental results a series of excitability curves showing the extent to which the three fundamental types of receptors of the retina are affected by light of any particular spectral colour.

Many writers have emphasised the existence of a white as distinct from a colour sensation, or the white arising from the admixture of fundamental colours. Objections to the trichromatic receptor theory have been based on the difficulty of explaining the peripheral and threshold white light sensation of the eye, and the absence there of all colour sensation. Captain Abney with others suggests that the retinal apparatus must be able to appreciate white light in addition to the three fundamental colours and their combinations, in which a white sensation must also be included. Hering met this supposed difficulty by introducing a theory which appeals particularly to the physiologist. His theory is based on the existence of three pairs of contrasting sensations, white and black, red and green, yellow and blue. According to Helmholtz, Hering's red was what is generally called purple. The first sensation of each pair is the result of katabolic action, that is, a dissimilative or molecular reduction of the living visual substance to a less complex and more stable condition, whereas the second sensation of each pair is due to a reverse anabolic or assimilative change of the substance to a more complex and less stable state. This theory presumes the existence in the retino-cerebral system of three kinds of living visual substance capable of responding katabolically or anabolically to the elementary light vibrations of each pair respectively. Hering evidently regards black as a colour sensation rather than the mere absence of light sensation such as a blind man is afflicted with.

Kries has advanced a duplicity theory which discriminates between the functions of the rods and cones. The former, which are more numerous towards the periphery of the retina, are concerned, he suggests, with achromatic scotoptic vision, that is, the vision of the dark adapted retina, whereas the cones are concerned with polychromatic photoptic or light adapted vision.

Donders elaborated the Young hypothesis by adding certain psychological or cerebral sensations which necessarily increase the speculative character of a theory.

There are numerous elaborations of these typical theories which need not be referred to, but it is desirable to mention the distinctive theory of Dr Edridge-Green, according to which the function of the rod appears to be to liberate, under the action of light, visual purple which spreads over the otherwise light insensitive cones which are stimulated, however, by the decomposition of the visual purple when light falls upon the retina.

Practically all the theories advanced concern themselves with the appreciation of colour and but little with the appreciation of form, except in so far as some writers consider it sufficient to state that appreciation of form is essentially equivalent to appreciation of colour contrast, black and white being regarded as colours. They have been established to a large extent upon the results of observations made with colour blind people of various types. Appreciation of form has received comparatively little attention from observers with the exception of Helmholtz, whose explanations have received general acceptance. In any theory of vision it is desirable that the appreciation of both form and colour should receive consideration.

Although Helmholtz's theory is so clearly explained in that classical work, the *Handbuch der Physiologischen Optik*, it is remarkable how frequently the theory is misstated. In view of this it may be of interest to quote the essential portion of Helmholtz's description, translated from the 1896 edition:

"When two light-emitting objects, whose widths are negligible compared with their separation, are observed, they can only be recognised as being two objects when between the retinal elements which receive their images there remains one which remains dark. The diameter of such an element must therefore be at least smaller than the separation of the two bright images.

From this description it will be seen that there must be a retinal element corresponding with the dark interval between the objects if the separate images are to be resolved, that is to say, in the resolution of two separate point images there are three features that have to be appreciated by the eye, namely, the two point sources and the intermediate dark space. Three retinal elements are therefore involved in the appreciation of the system. Helmholtz proceeds to consider objects whose width is equal to that of the dark strip between them, and concludes that all that can be said with certainty is that, at the limit of resolution, the retinal element must be smaller than the separation of the centre lines of the bright strips. This conclusion is based upon the assumption that a small part of the image falling upon one element will not be recognised if the greater part of the image falls upon the adjoining element, provided the quantity of light in the smaller portion is small compared with that of the latter. Doubtless this conclusion was considered necessary in view of the fact that there is a minimum limit to the diameter or width of an image that can be formed upon the retina owing to aberrational defects.

Neglecting the large chromatic aberrations, the bulk of which are not recognised by the retinal mechanism, as is demonstrated by Helmholtz, there is a well-defined enlargement of the image due to diffraction. Theoretically, for a normal pupil diameter of 4 mm. the angle subtended by the retinal image of a point object is about 1 minute, reckoned over the first dark ring. In the macula lutea the average individual cones also subtend an angle of one minute and as in this region they are in close contact, they are separated by this angular amount. Helmholtz accordingly assumed the resolving power of the eye to be 1 minute. This estimate is approximately in accordance with the results of contact observations. But as the diffraction image of a point object subtends about the

same angle, may not the results of these observations be merely a demonstration of this simple fact, and not of the actual resolving power of the eye, which may conceivably have no direct relation to the cone diameters? Is it possible in any case to produce upon the retina an image that subtends an angle of much less than 1 minute? Even if the resolving power were not determined by the diameter of the foveal cones, the relation of these diameters to the diameter of the maximum retinal image of an ideal point source may be of importance as determining the grain or texture of the retinal picture as distinct from the form. It should be observed in this connection that Helmholtz based his calculations upon the value of the cone diameters as determined by Koelliker, although the diameters as measured by other observers varied greatly, being in some cases less than one-third. This, however, does not negative Helmholtz's conclusions, since the contact resolving power of the eye as measured by various observers also varies considerably, although not to the same extent.

The accuracy of coincidence observations is well known. It is certainly less than one minute or even half a minute, and the precision with which a coincidence observation can be repeated is a question of seconds. It is not sufficient to say that coincidence observations are of a different type from contact or overlapping observations. That there are apparent differences is well known, although perhaps they are not so generally recognised in text books as they ought to be. For example, resolution of contact images is dependent upon the aperture of the optical system, whereas in the case of coincidence observations the precision of setting is not in the same way a function of the width of the aperture. If the objective which forms a fine line image in the coincidence field of view is reduced in diameter, the width of the image will increase, but it is about as easy to bring a thick line into coincidence as a thin one, and indeed the presence of fine diffraction marginal lines may even aid the coincidence observation.

Whatever the apparent differences between the various types of observation, it is desirable that any theory of the eye should take all aspects into consideration. Helmholtz himself considered it necessary to propound an alternative resolution theory, and it is interesting to note that this alternative theory is more frequently referred to by him in the later part of the work than is the former. In the section of the optic nerve where it pierces the choroid it is estimated there are about one million fibres. But the retina contains about three million cones and, neglecting the eighteen million rods which Helmholtz assumes may take no part in the resolution of form, it is evident that in the majority of cases several cones must be connected to each nerve. To meet this difficulty Helmholtz propounded his alternative theory. Suppose a layer of sensitive elements is overlaid by a network of interconnected nerve fibres; that this network on the one side is connected by nerve fibres to the more numerous sensitive elements of the retina, and on the other to the less numerous fibres of the optic nerve. Now if a retinal nerve element is excited, the effect will be transmitted to the nearest nerve fibres through several paths, and the relative amounts of the sensation in the nerves will depend on the respective lengths of the paths. The position of the excited element will then be appreciated by the

receiving mechanism with an accuracy determined only by the difference in intensity of sensation that can be appreciated.

In offering the following suggestions regarding the sensitive mechanism of the eye I have distinguished the appreciation of colour from the appreciation of form, and as regards colour I have adopted the assumption that the eye can recognise an impression of light, varying from a mere impression of grey white at the macula lutea to white light near the periphery, as well as three fundamental colours such as red, green, and blue and their combinations, including white light.

Practically all the reliable observational evidence so far accumulated points to the rods and cones of the retina as being the sensitive elements. It is convenient to distinguish by name the rods from the cones although in many respects they may have common characteristics and corresponding functions differing in degree.

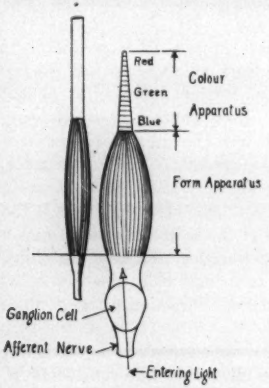


Fig. 11.

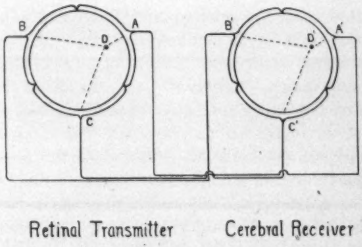


Fig. 12.

Fig. 11 represents diagrammatically a rod on the left side of the illustration and a cone on the right. Each element has two distinctive parts—a thicker inner portion facing the incident light and a thinner outer terminal portion. In the case of the cone the inner portion is ribbed vertically and the outer portion is ribbed horizontally. If these cones and also the rods are desiccated with sulphuric acid, the terminal portions separate into layers, thus indicating the existence of transverse septa. Horizontal layers have only been indicated in the outer portion of the cones, although actually they are present also in the rods. Further the two portions appear to have different compositions. The outer end of the cone contains the most highly refracting substance found in the eye. It is stained brown by osmic acid, which does not affect the inner portion. This latter, however, can be coloured with aniline dyes. There is obviously a strong distinction possible between these portions. The outer portion I shall call the form apparatus and the inner the colour apparatus.

Suppose, as indicated in Fig. 12, which represents a cross section of the form apparatus, that three nerves or sets of nerves *A*, *B* and *C* radiate from, say, the walls of the cone under consideration, and suppose that an entering ray of light traverses the point *D*. The action of the light upon the contained living substance is transmitted to the walls in a form capable of exciting the associated nerves. If the nerves *A*, *B* and *C* are connected to a similar cerebral mechanism, the apparent position of a point *D'* corresponding with *D* would be determinable with an accuracy independent of the diameter of the element and dependent only upon the precision with which the sensations transmitted by the nerves could be appreciated, that is to say, the position of a point might be located to a fraction of the diameter of a cone.

Although it is not essential to know the means by which the light vibrations are converted into a form capable of exciting the nerves, it is interesting to recall the theory recently propounded by Sir Oliver Lodge, who suggests the improbability of stimulations being due to the motion of any gross particles of the visual substance of the rods and cones under the action of the rapid vibrations of light. It is more probable, he thinks, that bodies moving more nearly in accordance with the velocity of light are involved. As is well known, the atoms of matter are now supposed to comprise a positive nucleus surrounded by a system of electrons of negative electricity revolving like planets round the nucleus with various velocities, the inner ones moving with velocities comparable with that of light. Under the cumulative effect of the light vibrations the equilibrium of these corresponding inner electrons may be so affected that they break loose and being projected from the atom, they may impinge upon the nerves disposed around the walls of the cell or dispersed throughout the mass.

A three wire system of the type illustrated in Fig. 12 for the location of position appeals to an engineer. If called upon to satisfy the conditions referred to, it is the system he would naturally adopt. But it is not possible to form an ideal point image on the retina, and even if a point image were formed it is possible that the cerebral apparatus would appreciate it as a minute area. The three wire system will only locate the centre of light intensity of any area upon which the light falls and it is possible that the brain would appreciate the area as a spot having its centre located at the centre of intensity of the image. Suppose for example the image of a broken edge falls upon the fovea centralis where the cones are pressed together, as in Fig. 13, and have presumably a hexagonal section, as has actually been observed to be the case in the fovea centralis. On the assumption that a fully illuminated cone will produce a cerebral impression of a spot of light, and a partially illuminated cone a spot of smaller diameter, proportional to the quantity of light falling upon the cone, and having its centre corresponding with the centre of intensity of the illuminated cone section, the cerebral appearance of the discontinuous line of Fig. 13 *a* will be as indicated in Fig. 13 *b*, from which it appears that a want of alignment of a fraction of a cone diameter can be recognised as a bend in the line. Instead of a sharp break the visual impression should be that of a rounded or oblique break. When the partial images subtend an angle of one minute

as in Fig. 13 *c*, the cerebral impression should be akin to that of a rhomboid, as indicated in Fig. 13 *d*. This is very easily tested by the observation of short coincidence lines and when this is done the rhomboidal appearance is indeed very striking. If the angular height of the lines is about 1 minute, it is hardly possible to distinguish between a want of coincidence and a simple rhomboidal form. The greater the want of coincidence the more oblique is the rhomboid, and the act of bringing short images into alignment really consists in the rectification of a rhomboid. This applies only to the case of very short lines subtending one or two minutes of angle, because when the lines are longer very close concentration is necessary to observe any rhomboidal appearance at the junction of the lines.

If the image of a grating in which the black and white spaces are about equal is considered it will seem that, when the spacings, as in Fig. 13 *e*, subtend about 1 minute, the cerebral impression should be that of a wavy line, Fig. 13 *f*, and under some conditions a knotted line. These impressions have been actually observed and recorded by Helmholtz. The same wavy appearance is often noticeable in observations of lines under natural conditions.

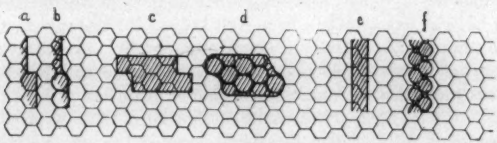


Fig. 13.

The function of the form apparatus is, I suggest, two-fold. Not only does it locate the position of the image falling within the confines of each cone of the macula lutea, but it also gives the impression of a grey or white light irrespective of the colour of the actual light. Over the central regions of the retina the form apparatus exercises primarily its function of locating the image, and conveys only in a secondary way the impression of light, which may under normal conditions be a negligible impression in comparison with the sensation of light transmitted by the colour apparatus. Near the periphery of the retina the rods preponderate greatly over the cones to which they have a characteristic resemblance. It is possible that the form apparatus of the rods may function primarily with regard to the impression of white light they convey, and only secondarily, if at all, with regard to the location of the image. Thus, neglecting the colour apparatus meanwhile, the central regions of the retina should recognise form well and the white impression of light only comparatively well, while the peripheral regions should recognise white light well and form badly. The small number of peripheral cones may aid in the detection of such form and movement as is recognisable over the outer regions. A gradual, although not necessarily regular, transition from the centre outwards might be expected in the case of both functions.

Now let us consider the outer or colour apparatus of the cones. A ray of, say, white light, which penetrates the form apparatus, will traverse the whole length of

the colour apparatus if of sufficient intensity and may pass through into the pigmentum epithelium, where it would be absorbed or utilised in other ways. It is reasonable to presuppose throughout the length of the colour apparatus a more or less continuous transition in the character of the visual substance as regards its response to light of the various wave lengths. Suppose therefore that the colour apparatus is divided into three portions connected to the brain by three nerves or groups of nerves respectively; that the outer or terminal portion responds to red light, the intermediate to green, and the inner to blue, these sensations being arranged in the order of the wave lengths to which they primarily respond. It will be understood that possibly no sharp line of demarcation between the compartments exists. In accordance with the recognised colour intensity curves, as for example those of König and Helmholtz, a ray of unsaturated red light would produce a large effect upon the red end; a small effect upon the green end and a negligible effect upon the blue or inner end. Captain Abney in his work on *Colour Vision* explains by the analogy of a double pendulum system how it is conceivable that more than one vibratory mechanism may respond to wave motion of a period with which they are out of tune. A ray of white light which passes through the form apparatus and traverses the whole length of the colour apparatus would stimulate all three sets of nerves and produce the sensation of white. A ray from the blue region of the spectrum would excite the inner end and to a small extent the middle section but would leave the extreme or terminal red end practically unaffected.

So far as colour appreciation is concerned there is an abundance of data already obtained from the observations of the various types of colour blind persons. The great majority of such persons are incapable of appreciating wholly or partially a red sensation although they can distinguish the other fundamental colours and their combinations. Cases of red and green blindness are not numerous, and the absence of all three colour sensations in a healthy eye capable of vision is a rare phenomenon.

On the basis of the proposed series theory the great preponderance of red blind cases might be explained as being due to the incomplete development of the terminal end of the colour apparatus and in consequence to the entire or partial absence of the visual substance that responds primarily to red rays, or to the absence of the associated nerves. Although incomplete development of the terminal portion of the cone is a probable explanation of red blindness, it will be evident that the series arrangement of the fundamental colour sensitive elements offers a variety of alternative explanations, especially if the cerebral mechanism is of the same character. If, on the other hand, one fundamental colour sensation were allocated to one particular set of cones it would not be easy to account for the absence of only those cones capable of appreciating say, red.

Alternative explanations on the series system are a possible absence or wrong distribution of the nerves of the various colour sections, or some incapacity of the visual substance to respond to light of certain wave lengths. Suppose, for example, that the development of the cone and the constitution of the visual substances were

both normal, but that the nerves were distributed more over the red and blue ends at the cost of the intermediate green section. In such a case the eye in question would be wholly or partially green blind to an extent dependent upon the defect of the nerve distribution. Lord Rayleigh in 1881 found that many people of normal colour vision so far as the recognition of the fundamental colours is concerned, require when making colour mixtures an abnormal amount of red or green or blue, as the case may be. Such anomalous trichromatic vision might be explained as being due to small abnormalities in the distribution of the nerves over the colour section or of the contained visual substances. For example, if the nerves were concentrated more over the blue end at the expense of the red and green, an extra admixture of these colours might be required when matching white.

There is an equally important series of threshold vision phenomena which must be explained by any satisfactory theory of vision. Abney describes a well known experiment which demonstrates what happens when a coloured light of diminishing intensity is viewed. Under a glass beaker covered with white filter paper he placed an incandescent lamp the current through which could be gradually reduced by means of a resistance. As the intensity was reduced, the yellow white changed to red and finally to a faint sensation of white free from any colour, although when the cover was removed the filament was still red. Placing a red, green, or blue gelatine cover over the lamp resulted in the same final white impression. As the result of other threshold experiments made with spectrum colours, Captain Abney in his work on *Colour Vision* states that, "Here then we have an indication that a person with normal vision passes through a stage of red blindness as the intensity is diminished before he arrives at absolutely monochromatic vision."

According to Abney, if the fundamental spectrum colours are viewed, and the intensity is continuously reduced, "the red becomes enfeebled before the green, then the red disappears while the green shines out brightly, and finally the green acquires a bluish hue." In experiments of this kind, the initial intensities of the various colours must be taken into account. At the fovea centralis, however, it is very difficult, if at all possible, to observe the final change to white light at the threshold of vision. Abney's statement is apparently not applicable to the foveal part of the retina.

Now to explain these threshold phenomena is ordinarily very difficult. Let us see, however, how they accord with the proposed series theory. A ray of white light enters, say, a cone and penetrates to the extreme end. All three colour sections are thus excited and the resultant impression is one of white light. If the intensity is reduced, the ray may penetrate the blue and green sections but fail to reach the red. At this stage the normal colour vision will have become red blind. Continued reduction of the intensity will result not only in red, but also green blindness, and ultimately in total colour blindness. At this stage there will only be an impression of light resulting from the excitation of the inner or form portion of the cone or rod. Further reduction of the light results in darkness.

Since the form apparatus is still excited when the light is too feeble to penetrate the colour apparatus, it could be prophesied that the perception of form would

persist to the end, and further, since the location of a point image by the form apparatus is dependent upon the ratio of the sensations in the three nerves, it is to be expected that the accuracy of coincidence observations would, generally speaking, be independent of the intensity of the light. So far I have not been able to detect any important differences in the precision of coincidence observations when using lights of different colours, and the precision certainly does appear to be independent, within limits, of the illumination. In certain experiments a reduction of the illumination of 1000 fold resulted actually in a small improvement of the precision. But accurate form observations cannot apparently be made at very low intensities in the neighbourhood of the threshold. This is probably due to the quantity of light near the threshold being insufficient to excite the reflex mechanisms that control the accommodation apparatus of the eye, in consequence of which it is not possible to keep the images in focus.

As the intensity of white light is reduced, the quantitative effect upon the terminal red end will also be reduced, with a consequent shift of the mean intensity position towards the green blue end. This shifting of the intensity towards the blue end as the quantity of light is reduced was observed by Purkinje, whose name is now generally associated with the phenomenon.

Reference has already been made to the fact that over the fovea centralis there is no appreciable change from coloured light to white light at the threshold of vision. It will be recollected that in the fovea centralis there are only cones, and that these cones differ appreciably from those distributed over the more peripheral parts of the retina. The foveal cones appear to be more slender and their terminal colour apparatus portions are lengthened at the expense of the inner portions.

That there is a great similarity between the rods and cones is evident and it is reasonable to expect that in many respects their functions may be similar, although they may differ in amount and in some cases be entirely absent. It should not be forgotten, however, that, so far as is known at present, the rods alone appear to be directly associated with the secretion of the retinal purple which was first demonstrated by Boll in 1876. In view of the entire absence of rods in the fovea centralis and in view of the fact that over this area coloured lights do not finally appear white when the threshold of vision is reached, it is a reasonable conclusion that the absorption of the light in the cones may be small compared with the absorption in the rods which predominate in other parts of the retina. Thus in the case of the rods, red light of low intensity might be all absorbed in the form section of the rod and thus finally appear white, whereas in the case of a cone a small difference of intensity would not be recognised in the same way. It will be recollected that at the fovea centralis the overlapping portions of small images did not appear more intense than the remaining portions.

It is not necessary to discuss the question of complementary after images which can readily be explained, but it is desirable to refer to the subject of contrast colours, the explanation of which is usually avoided by ascribing them to psychological impressions. Contrast colour phenomena necessarily imply some interconnection of the various parts of the retina. A similar interconnection is

suggested by the reflex actions that depend upon the total quantity of light falling upon the retina and necessitate some integrating device. Now the rods and cones are not connected to the brain by simple continuous nerve fibres; the connection is a highly complex one. It will be recollected that the nerves from the rods and cones pass to the surface layer of the retina through three clearly defined layers of ganglion cells separated by distinct layers of fine molecular substance. This was illustrated in the second lecture by a diagrammatic section of the retina. Such an arrangement is an ideal one for the fulfilment of the conditions described. Suppose the nerves that control the reflex mechanism are associated with the three layers of ganglion cells. Suppose also that the nerves from the red sections of the rods and cones are connected to the first layer of ganglion cells and those from the green and blue to the second and third layers of ganglion cells respectively. If now the cells of each layer are interconnected by nerves associated with the accommodation mechanism, then the excitation, say by red light, of one or several parts of the retina would result in the stimulation of the corresponding portions of the ganglion cell layer with a consequent indirect excitation of the associated nerves controlling the reflex action. It should be understood that the stimulation of these nerves may be of quite a different character from that of the visual nerves. The total quantity of the red stimulus would thus be integrated and determine the amount of the reflex action. Similarly the green and blue ganglion layers would integrate the effects of these colours or their combinations. But if the ganglion cells of each layer are indirectly connected in this way, it is conceivable that the visual stimulation of one group of cells may have a suppressive effect upon adjacent groups through which the transmission of visual stimulations would be reduced. Thus, suppose one retinal area is stimulated by red light and an adjacent one by white light, it is conceivable that the passage of the red light stimuli through the corresponding ganglion cells would react upon the adjacent interconnected cells of the same layer, through which the red elements of the white light stimuli are passing, and tend to suppress their activity so far as the transmission of the red impulses is concerned. As the activity of the green and blue ganglion cell layers would be unaffected, a partially red blind impression of the white light would result, as is approximately the case in contrast colour phenomena of this kind. The intensity of the red area itself would be slightly reduced by a similar reaction but its colour would still be red, and the red blind impression of the white would not be affected. It will be understood that the reaction referred to is probably of quite a different character from the light impulse transmitted through the ganglion cells by the nerves.

Irradiation can hardly be attributed to defective insulation of the rods and cones in view of the presence of the pigmentum epithelium. If the above assumption is accepted, it seems more probable that irradiation may be due to defective insulation of the ganglion cells so far as the direct light stimuli are concerned.

In view of the fact that the optic nerve contains only about one million individual nerves, it may be objected that to allocate six nerves to each cone of the fovea centralis and possibly three colour nerves to many of the remaining rods and cones is not justifiable. There are about three million cones in the retina and about

eighteen million rods. In the more peripheral parts it is often assumed that the elements may be grouped. But although in the optic nerve there are one million individual insulated nerves, there is little justification for asserting that each is an elementary nerve. The axis cylinders of the individual nerves appear to be bundles of fibres, and although little is known of their details or the extent to which the fibres are insulated from one another, there is good authority for the suggestion that they may be capable of transmitting separate stimuli. The multiple character of the nerves is also suggested by the way in which the fibres of the axis cylinder ramify through the substance of the neurones and recombine to form other nerves.

Much experimental data regarding the detailed performance of the human eye has already been accumulated by many separate investigators, but very much more is necessary. Continuous organised research, not only of the functions of the eye, but also of its histological details, is essential, and I sincerely hope it may be possible in one or other of our leading technical institutions, where such combined effort is alone practicable, to allocate to this important work a sufficient number of research scholars.

Erratum in "The Unaided Eye," Part I. On pp. 224, 225, and 229 the values of x in the equation Area $= K$ (Illumination)^x should in all cases be *negative*.

DISCUSSION

Professor Cheshire expressed his admiration of the apparatus employed by the author in obtaining the results he had shown. The importance of these in their application to rangefinders was evident, and they went far to explain the advantage which the coincidence type of range-finder had in practice been found to possess. He should like Mr French to make one extension of his observations, and repeat Helmholtz's study of coincidence settings with objects at different distances, using stereoscopic vision. At the time those experiments were made the importance on the result of some of the precautions which later workers had taken was not realised. It would be most valuable if Mr French would obtain corresponding results for stereoscopic settings, and he hoped this would be done and the conclusions described to the Society.

Mr T. Smith (remarks partly communicated) asked the author's reason for adopting an unusual form for the expression of the errors of observation. The introduction of new methods and terms was somewhat confusing and the value to be attached to them was not at once evident. Regarding the experiments referred to in the paper, was the accuracy of setting the same when the object consisted of a white line on a black background as when the line was black and the background white? If not, which was the better and by how much?

He was greatly interested in Mr French's theory of vision and admired the skill with which it had been presented, which lent it such an air of plausibility. As he did not labour under the disadvantage of having studied any theory of vision,

he felt no diffidence in raising objections to the author's theory and suggesting an alternative. There were really two separate theories, the one dealing with the perception of colour, the other with form. The suggestion that the recognition of colour should be associated with the stratified ends of the cones had been previously made before the Society by Mr Chalmers, who however associated the smaller discs with the shorter wave-lengths, the larger ones with longer wave-lengths. The theory presented difficulties to his mind, but he proposed only to criticise the theory for the perception of form. The author's task is essentially to offer some explanation of the fact that, while the resolving power of the eye, the size of the nucleus of the image of a point or line source, and the spacing of the sensitive elements on the retina are all about one minute, the collineation of two lines, a systematic error apart, can be repeated again and again within a fraction of a second. The attempts to reconcile these two figures has led to a suggestion which involves a complex mechanism within elements which are themselves extraordinarily minute, the aim of which is the location of the centre of intensity of luminous radiation within a small fraction of the diameter of the element. Doubtless the theory cannot be rejected on the ground that the presence of such mechanism within so small a space is incredible, but it is quite pertinent to ask whether it is necessary. Incidentally this theory greatly increases what is in any case a difficulty, the disparity between the number of recording elements on the retina and the number of fibres in the optic nerve. Admitting the author's suggestion, the subdivision of the fibres must have a finite limit, and the more a theory strains our ideas of the fineness to which the subdivision may be carried, the less must we regard it as likely to be true.

Objections to the proposed theory, however, need not rest upon such debateable ground. On closer examination it is seen to fail where at first it appears most successful. Take for instance the wavy appearance of a straight line, illustrated in Fig. 13 *f*. The "wave-length" on this theory is essentially of the same order of magnitude as the spacing of the cones, which agrees with the resolution limit. Is it credible that so fine a corrugation would be appreciated by the eye as waved? Even more is it not an absolute contradiction on the author's theory to admit that a straight line appears waved with corrugations about a minute deep and yet claim for the eye ability to detect a want of alignment so much smaller*?

A still greater difficulty arises when the results of the theory are examined quantitatively. The three wire system locates, to a fraction of a second, the position of maximum intensity of the diffraction pattern of the image. The distribution of light intensity in this pattern is known, and the necessary sensitiveness of the system to variations in intensity can be calculated. The speaker has made such calculations—for the sake of ease of calculation they were for a rectangular aperture, but the order of magnitude will be the same for a circular aperture. Taking one minute as the width of the central band of the image, the mechanism must reject as incorrect a point where the intensity is less than the maximum by 1 part in 1090 for an accuracy of setting of one second, and by 1 part in 4360 for an accuracy of

* *Note added later.* Unless it can be shown that the waviness described by Helmholtz has this very small period, it is not explained by the proposed theory.

half a second. The possibility of detecting such small variations is undoubtedly negatived by our knowledge of the accuracy attainable in photometry.

By considering the diffraction pattern of a double line it may be shown that the above figures involve ability to resolve lines closer by about 15 per cent. than is usually supposed*.

On seeking for an alternative explanation free from these defects, the suggestion which seems simplest to the speaker is one which does not modify the prevalent view that an individual rod or cone will simply transmit to the brain the information whether visible radiation is falling upon it, its intensity, and possibly its colour, but with no finer localisation than the size of the rod or cone itself implies. The smallest detectable difference of intensity would not be less than that found in photometric measurements. The limits for resolution, which are satisfactorily in accord with theory, are unaffected. The explanation of the very fine setting for alignment should be sought on the supposition that the eye makes small movements in its socket rather than by assuming it to be quite still. Such movements take place without the knowledge of the subject, but conscious effort may easily alter the character of these movements. When attention is directed to ascertaining whether two lines are accurate continuations of one another, these small movements may be restricted so that the image of the line makes no side movements relatively to the retina. As soon as there is any consciousness of change in the illumination of any elements in the neighbourhood of the image, an appropriate corrective movement is automatically applied. If the lines do not join accurately, the change of illumination at that point will be noticed. The high degree of accuracy is readily understood if it is assumed that the sensitive elements on the retina are not necessarily symmetrically distributed, so that the line will just touch some elements and just miss others. This may be facilitated by ability to alter slightly the orientation of the eye, so that the elements are arranged to give the greatest exactitude in such a process. More exactly, since there is no hard line of division between light and dark in the image, the observer will be conscious of a change in the amount of the light falling on the element. The accuracy of setting will thus be effected, not at the centre of intensity, but at the margin of the diffraction band where the intensity changes most rapidly. This would explain why the change from a thin to a thick coincidence line is not greater. As the line broadens from being very fine to a considerable thickness, the diffraction pattern changes from that of a fine line to that of an infinite plane divided by a straight line into dark and light regions. It would be interesting to compare Mr French's curves with calculations on this basis. The experimental results recorded in Figs. 3 and 4 are, qualitatively at least, of the character that this theory would lead one to expect.

Such a theory is compatible with various theories of colour vision. It is unnecessary to suggest one rather than another, but it may be pointed out that small eye movements may themselves be introduced as a not unimportant factor in a colour theory. We may suppose that each element responds to a range of colours corresponding to only one of the fundamental sensations, whether by a filtering

* *Note added later.* On this particular point there is no objection to raise.

of the light before the sensitive element is reached, or by ability to respond only to vibrations about a certain mean frequency, or by other means, and that in a small area of the retina capable of colour differentiation a number of the different elements are indiscriminately mixed up in the same kind of way as starch grains differently stained are packed together on an autochrome plate. The brain may then interpret the results either by integrating over a small area of the retina or preferably by recording the mean impression over a certain interval of time during which elements susceptible to different spectral regions have swept over each portion of the image. Methods of testing such a kinematic theory of vision, either as regards form or colour, in comparison with a static theory should not be difficult to devise, and the results would undoubtedly be of sufficient interest to justify the attempt.

Mr L. C. Martin (communicated remarks): I wish to express my appreciation of the careful and important experimental work of Mr French. I cannot agree with his explanation of the difficulty of form observations near the threshold (given on page 145). My ordinary experience of ill-defined outlines of dark objects seen on a dark night, while stars are seen perfectly focussed, seems to negative such a suggestion. Further it does not seem to me that the phenomenon is at all difficult to explain on the "interconnection theory."

I should like to suggest that the possible discrepancy between the number of retinal cones and optic nerve fibres does not necessitate interconnection of the cones in the fovea where *alone* clear form vision is possible. Interconnection probably does not exist outside the fovea, and the late Mr Chalmers* gave a very ingenious explanation of threshold phenomena on such an assumption—but foveal threshold phenomena are not well enough known to justify any opinion at present.

Instructor-Commander T. Y. Baker (communicated remarks): The Society is very much indebted to Mr French for the extremely able and interesting series of papers on "The Unaided Eye" now just completed. I cannot help thinking, however, that the author has, during the whole series, been desirous of showing how accurate the eye can be in making an adjustment for coincidence, a desire which perhaps is only natural under the circumstances.

The figures that Mr French gives to show what the eye can do in making this setting, appear, at first sight, to be very much below those found by Dr Bryan and myself in 1911. The casual reader of our paper at the last Optical Convention would receive the impression that the unaided eye can set within an error of approximately 10 seconds; Mr French's paper would suggest to him that the figure was about half a second. This large discrepancy arises partly in this way. Mr French's observer makes five settings for coincidence and the "mean difference from the mean" works out at half a second. There is not, except in one case, any indication as to what the actual errors in setting are. As against that, in the Greenwich experiments, the mean error only was retained except in one case. The comparison of 10 seconds against half a second is therefore not a fair one to make. Incidentally, it does not appear quite evident what justification there is for accepting the "pre-

* *Trans. Opt. Soc.*, 20 (1919), 297.

cision," as Mr French obtains it, as a criterion of what the eye can do. If a man counts a thousand sovereigns five times over and makes his counts 988, 992, 991, 989, 985, his "precision" is 2; but has the term any practical meaning? It may be argued that precision as applied by Mr French does connote a definite accuracy because in the use of a range-finder if the observer also adjusts for infinity, he will make the same kind of error there, and, subsequently, his readings will be correct within the "precision" and not within the "adjustment" only. I should be very dubious about accepting any such statement without evidence that in the generality of cases an observer's "precision" and "adjustment" were the same on the artificial infinity setting as during the ordinary work of the range-finder.

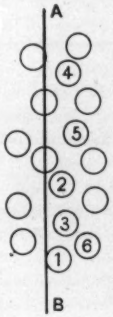
I do not, in fact, consider that it is fair, when speaking of the capabilities of the eye in such circumstances, to deal with anything but the actual errors which are made, and the quantities connected with them. If that is done, I should imagine the errors of observations obtained by Mr French would work out at one half or one third of those obtained by Dr Bryan and myself. That difference I put down to (1) the higher degree of refinement of Mr French's apparatus (our experiments were carried out under somewhat poor conditions of illumination of the target and observers on several occasions complained of eye-strain) and (2) the greater proficiency and skill of Mr French's subjects who, I suspect, were men who had had years of training on this type of setting, whereas some of the men we tested had never even seen a range-finder.

As I stated during the discussion, I think the terms employed in describing the acuity of the eye in this respect are somewhat liable to misconstruction. It is so easy to infer that, when Mr French states that the precision of "A" is 0·49 sec., the error with which that observer repeats his observation never exceeds that figure, whereas stated in the generally accepted terms of the theory of errors of observation 0·49 sec. is his "mean error" and 0·41 sec. the "probable error," i.e., an error of such magnitude that it is an even chance whether it is exceeded or not. An analogous quantity is used in ballistics when dealing with the fall of shot and is termed in that case the 50 per cent. zone, the double breadth being measured. The 50 per cent. zone in *A*'s case would therefore be 0·82 sec., and this represents the space within which 50 per cent. of the errors may be expected to lie. That is very far from representing a space which includes the whole of the errors. If a zone is required which covers 90 per cent. of the errors, we must multiply the "precision" by a factor 4·1. The 99 per cent. zone requires the factor to be 6·5 and the 99·9 per cent. zone the factor 8·3. If a 99 per cent. probability be looked upon as near enough to absolute certainty, then it can be said that "A" repeats his readings within a space of 3·1 secs.

The question of nomenclature in cases such as Mr French has dealt with is really so important and the terms are often so loosely used and so liable to misinterpretation, that I feel I may be pardoned for raising the point and for pleading for something in the nature of a standard notation.

Dr R. S. Clay said that he agreed with Mr Smith in doubting the theory put forward by Mr French for explaining the high accuracy with which the eye can set

two lines in alignment. He thought it much more probably due to the slight irregularity of the positions of the rods and cones. Suppose *AB* a line of light falling on the retina and suppose the circles to represent rods and cones. If now *AB* is moved slowly to the right, the light would fall successively on those circles numbered 1, 2, 3, 4, 5, 6, and thus its movement would be observed even if the movement were only about one-sixth of the diameter of a cone. The larger the line, the more accurately this successive stimulation would allow its position to be recorded. This would also explain why practice would enable its position to be more accurately obtained, for by repeated experiments the brain would learn to record and interpret the successive stimulations with greater facility.



Mr French's explanation of colour effects in weak light was ingenious, but he thought that the ordinary physical laws applied to resonators subject to a certain amount of damping would explain the results obtained. If a very weak periodic disturbance acted upon a damped resonator, the response of the resonator would be only slightly affected by comparatively large changes in the period; in other words, if the damping absorbed most of the energy communicated to the resonator, the tuning would be very poor. Thus for instance a very weak red light might stimulate all three colour sensations almost equally. He did not see how Mr French could expect the absorption in a fraction of the length of a cone to do all he attributed to it after it had traversed unharmed all the other numerous layers of the retina.

Mr Guild (communicated remarks) said he was struck by the precautions which had been taken to eliminate the subconscious dishonesty by which an observer's readings might be effected. Such precautions were no doubt essential when the main object of the observations was to determine a property of the retina. From the point of view, however, of comparing the accuracy of different types of setting, measurements should be made under the circumstances which would usually occur in practice in which the observer's dishonesty has its usual scope. The tendency to self deception, and the influence which previous settings have on the next is a very real difficulty with most observers, and certain types of setting are much more liable to be affected by it than others. Consequently it has to be taken into account in considering the suitability of settings, and comparative measures are rendered *less* valuable if elaborate precautions are taken to keep the observer from knowing what he is doing, since such devices are impossible under the conditions in which most instruments are used.

The effect of astigmatism on coincidence settings is a serious matter. It detracts enormously from the advantage which the setting possesses of high precision in repetition. It would be interesting to have a comparison of the effect on other types of setting employed in accurate work.

Mr French's experience that the sensitivity was not affected by employing very feeble illumination is surprising in view of the fact that visual acuity falls off con-

siderably as the illumination is lowered. It appeared from this that the distinctness with which the lines were seen did not affect the precision of the setting.

The series theory of colour vision is most interesting, but when examined in detail some of the deductions from it clearly require modification. For instance the explanation of the Purkinje effect and the ultimate passage to colourless vision at low intensities as due to the rays of longer wave-length being unable to penetrate to the far end of the cones would require an enormous absorption coefficient when the small thickness of the material is taken into account. But the material of the cones is practically colourless and transparent.

Dr J. S. Anderson (communicated remarks): It is interesting to note that many of the author's results confirm those obtained by Dr Hans Schulz* in connection with an investigation into the accuracy of coincidence measurements in range-finders. Mr French, however, seems to obtain a much smaller value for the physiological limiting angle than Schulz's value of 10 seconds, which agrees with the results obtained by Commander Baker and Dr Bryan in 1911. An abstract of Schulz's paper will be found on p. 167 of this number of the *Transactions*. I think Mr French's method of attacking the problem is much to be preferred, as the method of determining the percentage of correct estimations of arbitrary displacements necessitates the making of a large number of observations before the theory of probability can be applied. Schulz's results are, however, of interest, as they represent the mean values obtained by over 50 trained observers.

Major E. C. Henrici (communicated remarks): The results of Mr French's tests on the accuracy of setting coincidence lines are most interesting, and the errors obtained are most unexpectedly small. The results are of great importance from the point of view of the accuracy obtainable with various measuring instruments, and therefore of the design of instruments for accurate measurements. It is to be hoped that Mr French will be able to publish further figures and details of his tests. Mr French gives 9 sets of 5 readings each of setting a coincidence line, but does not give full particulars as to how these sets differ from each other—were they taken by different observers and was the moveable line always brought up to coincidence from the same side? In accurate measurements one would naturally take an equal number of observations bringing the moveable line into coincidence from each side, when such a procedure is possible, as it generally will be, though not in the case of range-finders. This point has been dealt with to some extent later in the paper, and the results seem to differ very considerably. The results given in Fig. 6 show a systematic error of about 20 seconds in the best case, while those of Fig. 7 show only about 2 seconds—is this the difference between two observers, or were the conditions different? The systematic error of 2 seconds is comparable with the accidental error, that of 20 seconds is not.

The method of calculating the "precision error" adopted is one that will make this error appear a minimum. The error given is the average deviation from the mean of five observations, and though this is theoretically 0·7979 of the root mean squared error (or as I prefer to call it for short the "mean error") it is generally

* Dr H. Schulz, *Z. f. Instrk.*, 39 (1919), 91, 124, 242.

less than this when only a small number of observations is considered. The mean errors for sets A, B, and C work out at 0·69, 1·08, and 0·89 seconds respectively, while taking these as one set they work out with a systematic error of 1·01 and an accidental error of 1·12 seconds.

It would be most interesting if Mr French could publish further particulars of his numerical results; the ones given were admittedly taken in ideal conditions which would never be obtained in practice, and very few figures are given to show the effect of other variables, and how far different observers, with what is generally considered good eyesight, vary among themselves.

It is also to be hoped that further observations will be taken with other forms of intersection, e.g. a line bisecting a bulls-eye or fine parallel lines set over a central line. The apparatus Mr French has constructed seems ideal for this purpose, and if he is unable to spare the time for this work perhaps the research could be undertaken at the Imperial College, or elsewhere.

The author communicated the following reply (August, 1920). Owing to my absence on the Continent for several months I very much regret that circumstances have prevented me from replying as fully as I should have liked to the very interesting criticisms of the members and from carrying out, as I intended, the Helmholtz experiments suggested by Professor Cheshire.

In reply to Mr T. Smith, similar observations, although not so many, were made with white lines on a black background. The results were characteristically the same but generally speaking the white line results were about 5 to 10 per cent. less accurate than the black line results. This, however, may possibly be due to the greater interest paid to the black line experiments.

While it is quite true that others, prior indeed to the late Mr Chalmers, have suggested that the stratified ends of the rods or cones may be associated with the appreciation of colour, Mr Smith is hardly justified in regarding the theory as two separate ones dealing with the perception of colour and form respectively. To my mind the essential, and, so far as I know, characteristic feature is the series arrangement.

Mr Smith states that on closer examination my theory is seen to fail where at first it appears most successful, but I must confess a feeling of disappointment that Mr Smith in support of this statement has contented himself with merely asking, is a certain appearance credible? As the wavy appearance is one recorded by Helmholtz, it is quite credible. I, too, have certainly noticed the same kind of effect.

Mr Smith has calculated the theoretical width of the diffraction image and drawn conclusions with which I can hardly agree, but in any case the objection does not appear to be so very serious in view of his statement that "these figures involve ability to resolve lines closer by about 15 per cent. than is usually supposed." Considering how much depends upon the particular observer, there is nothing very remarkable about that.

With regard to the last paragraph of Mr Smith's statement, I presume Mr Smith's

own equipment at the National Physical Laboratory is most suitable for such work, but I think his task will prove a difficult one. It is much easier and indeed safer first to undertake the work of obtaining measurements and then of interpreting them rather than to propound a theory and then attempt to obtain results in support of it.

In reply to Mr Martin, at the threshold of vision a point source appears to me to broaden out and become an ill-defined patch of bluish light. That may be a peculiarity of my eye; on the other hand, in view of Mr Martin's description, it is probable that my threshold illumination was very much less than the ordinary dark but starlight night illumination.

I quite agree with Mr Martin that the allocation of one nerve to each cone of the fovea centralis does not much affect the average interconnection for the vast number of remaining elements, but the fibrous nature of the axis cylinder of the nerves appears to be so definite that I did not hesitate in going farther and allocating a series of fibres to each foveal element at least.

Commander Baker has, I think, overlooked Table II which gives the extreme readings of each precision set, of which full particulars are given for nine sets, A to I, in Table I, not merely one set, as Commander Baker states. These nine sets were really not made with any record-breaking intention. They were actually made by a lady who, although an excellent scientific observer, is not a skilled coincidence range-taker and has no special skill in such work. Every reading taken is included in the series as published. I have little doubt that with a selected operator still better readings might be obtained. In my opinion Table II is more interesting than Table I and should not be overlooked.

In reply to Mr Guild, I quite agree that in the first instance it is desirable to conduct practical experiments under the natural conditions. In coincidence range-finding that, however, has been done many times. I have access to thousands of observations made under all kinds of conditions. Having this information, the next stage is the allocation of these errors. This necessitates the elimination one by one of the sources of error. In my experiments I have eliminated as many as possible of the external sources and endeavoured to show that of the final error a considerable portion is attributable to optical defects such as astigmatism.

There is no suggestion in the lecture that the observer with all his defects of vision can observe to the amount of the precision error. Indeed the combined adjustment plus precision error may be exceeded when the operator is asked to state whether or not two lines are in coincidence instead of being permitted to bring the lines into coincidence. In the latter case the operator may unconsciously make some allowance for his known adjustment and premature adjustment errors.

With regard to the last part of Mr Guild's criticism, may it not be possible that fibrous or other layers between the septa affect the absorption?

Several members prefer that the precision should be calculated on the basis of the theory of probability. The system I have used is the one now generally

adopted in such work. It has the merit of simplicity and under the conditions of such work it is just as accurate. Professor Mellor in his *Higher Mathematics for Students of Chemistry and Physics* states:

> As a matter of fact the theory of probability is of little or no importance when the constant or systematic errors are greater than the accidental errors.

The nature of the mean error was particularly described and illustrated in the lecture and I cannot see why there should be room for confusion as to what was meant.

Note added later. I understand from Mr Smith* that he regards the photometric objection as being of principal importance. In this connection I would point out that in most of the coincidence experiments made by me the image lies within the area of the macula lutea and in the majority of cases on the fovea centralis itself. To apply ordinary photometric observations that concern large areas of the retina to minute areas such as the fovea centralis does not seem to be advisable and, so far as I know, there is very little information regarding photometric values for such areas. Such observations as I personally have made seem to indicate that the general results are not applicable, and to attempt to draw any conclusions from such data is hardly possible.

* See footnotes on pp. 148, 149.

THE ROCK CRYSTAL OF BRAZIL

By ROBERT R. WALLS, M.A., B.Sc.

MS. received, 10th February, 1920. *Read and discussed, 15th April*, 1920

In the summer of 1919 the writer made a trip to Brazil on behalf of Messrs Adam Hilger, Ltd. in search of pure rock crystal for optical purposes. The properties and uses of rock crystal are too well known to need further discussion here, but a few notes as to its mode of occurrence might be of interest to the Optical Society.

Previous to the war the rock crystal of Brazil had been worked almost entirely by Germans. During the war the production had been neglected or mismanaged, with the result that when the writer arrived in Rio de Janeiro there was practically no good crystal to be had. It was necessary to make a long journey into the interior

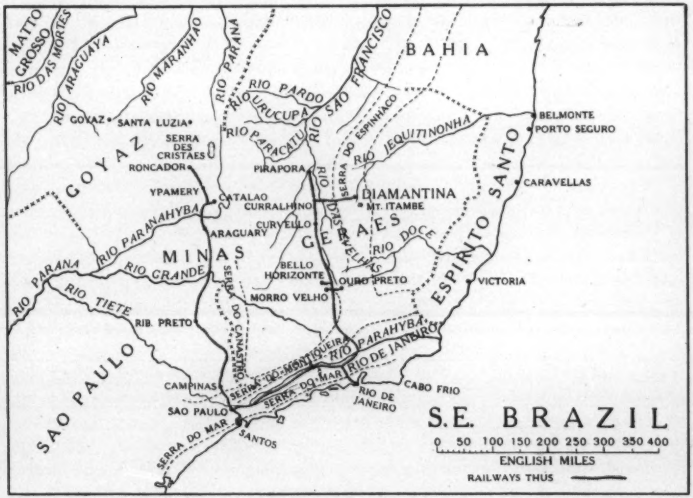


Fig. 1. Map of S E. Brazil.

to look for it. Enquiries brought the names of four regions where crystal had been worked commercially. These were (1) near the town of Diamantina in the State of Minas Geraes, about five hundred miles due north of Rio; (2) on the Serra des Cristaes or Crystal Mountains in the State of Goyaz in Central Brazil; (3) in the State of Bahia in Eastern Brazil; and (4) in the State of Sao Paulo in South Brazil. The first two districts appeared most promising and journeys to these districts were resolved upon (Fig. 1).

Before proceeding farther a few explanatory notes on the occurrence of quartz and the geology of quartz-bearing rocks might be very appropriate. Quartz (oxide of silicon) in one or other of its many forms is one of the most ubiquitous minerals in the earth's crust. As veins of white or milky variety it radiates from granite masses through the surrounding rocks in all directions. Occasionally it is found in small quantities of a fine yellow or blue colour when it is known as cairngorm and amethyst and is valuable as a precious stone. Amorphous forms such as chalcedony, agate and jasper (bloodstone) are found in veins and seams of rocks under similar conditions. Flint and chert are found as nodules and concretions in chalk. Another amorphous form containing water of crystallisation is known as precious opal.

Igneous rocks which contain mineral quartz as a primary constituent are termed acidic. Of these the best known is granite, a rock consisting essentially of quartz and felspar with smaller quantities of mica, tourmaline, etc. Rocks containing no free quartz but a large proportion of alkaline earths and heavy metals are known as basic. Intermediate rocks contain varying proportions of silicates. This classification depends entirely upon the chemical characteristics of the constituent minerals of the rocks.

Amongst derivative rocks the chief quartz-bearing example is sandstone. A pure sandstone is built up entirely of minute quartz grains compressed and cemented together. Several organic rocks are built up of the siliceous shells of minute organisms (radiolaria). Quartz grains are found in practically all derivative rocks, and quartz in the form of sand covers great areas of the sea floor.

The clear glassy quartz of Brazil—known commercially as Brazilian rock crystal—occurs in large well-formed crystals. Now large crystals, as every student of elementary chemistry knows, are a sign of very slow cooling and slow crystallisation. Crystals of Brazilian quartz therefore, such as were commonly brought into this country for commercial purposes, must have taken many years, possibly hundreds of years, to crystallise and reach the form in which they were found. Naturally, therefore, anyone prospecting for rock crystal would be inclined to search for it in an acidic rock which had been formed by long and slow cooling from a molten condition, and in which the crystals were therefore large. Granite in some form is the only rock which satisfies these conditions and the crystal would therefore be looked for in nests in the granite or as veins and seams of quartz from the granite permeating the surrounding country rock. Strange to say the striking features of the scenery of Brazil, especially in the neighbourhood of Rio de Janeiro, are the huge mountain ridges and bosses of granite.

The writer spent a fortnight examining these granite masses in detail. Several varieties of granite could be distinguished. The underlying granite everywhere had been sheared and distorted to form a coarse gneiss. Sometimes there was a clear line of demarcation between the granite proper and the gneiss but this was not always noticeable. The gneiss was composed of large crystals of felspar—generally about three or four inches long—black tourmaline, and milky quartz in minute crystals filling up the interstices. The granite was composed generally of the same minerals but in much smaller crystals. Generally speaking the gneiss appeared brown in colour while the granite on a fresh exposure was grey.

The rock everywhere was weathering rapidly and giving rise to a thick clay on which the rich tropical vegetation flourished. On the Corcovada, a mountain over two thousand feet high, on the outskirts of the town, a bare rock face could be dug into for a depth of several inches with a lead pencil. From the weathering of so much rock great quantities of fine quartz sand are produced and there are some fine stretches of sandy beach on the Atlantic coast. In all the rocks round Rio de Janeiro, however, the quartz is milky in appearance and occurs only in very minute crystals filling up the interstices between the other minerals exactly as it is found in the granites of this country. The clear glassy rock crystal could nowhere be found.

The first trip which the writer made to a reputed crystal district was to Diamantina in the State of Minas Geraes. The journey could be made in two days of continuous travel by rail, but the writer took five days, stopping to note the various changes in the geology of the country passed through. The town of Diamantina lies on a ridge of hard sandstone, the Serra do Espinhaco or Backbone Ridge which stretches in a north-south direction for a distance of nearly five hundred miles with an average width of nearly fifty miles. The height of this ridge, near Diamantina at least, is about five thousand feet, and here it gives rise to the Jequitinonha River which reaches the Atlantic Ocean after a course of over four hundred miles. The town of Diamantina owed its origin to the discovery of gold in the neighbourhood, but the subsequent discovery of diamonds was of far greater importance and gave the name to the town. Besides diamonds, aquamarines and crystal are found and the Jequitinonha River and its tributaries are rich in these minerals.

Most of the crystal of commerce had been collected from the river bed. The writer spent a day or two searching in the neighbourhood of the town and did find a few small round pebbles which when broken with the hammer proved to be beautiful clear crystalline quartz. But they were too small to be of much commercial value, although they were being cut by local lapidaries and sold as precious stones.

Shortly before the war a German expedition had started from Diamantina and travelled down the river, turning over the loose gravels and surface deposits and collecting crystal and precious stones which the river had brought down. Occasionally when they came across particularly rich deposits they hired local black labour, which is cheap, and made more extensive excavations. They were last heard of, when war broke out, at a point about two hundred miles below Diamantina and by that time they had made a rich harvest of minerals. Incidentally they had so cleared out the river that the writer decided it was no use hunting there for crystal in commercial sizes and quantities and determined to hunt back to the sources of the rivers and find the crystal in the rock where it had originated.

This involved a close study of the sandstone cap of the Serra do Espinhaco. The results of a rough geological survey of the district indicate that the sandstone consists of a series of horizontal beds and lies directly on the fundamental granite and gneiss. The sandstone has many remarkable local modifications. In places it shows ripple-marks, indicating the shallow waters of the sea or lake in which it was laid down. Sometimes the sandstone is friable and chalky and easily pulverised.

This last variety is typical of places where quartz veins from the underlying granite have pierced the sandstone vertically, intersecting the horizontal bedding. These quartz veins are often auriferous and practically all the old gold workings are on the site of such veins. There was no sign of "rock crystal" near any of these veins, however.

Over large tracts of the ridge the sandstone is baked hard and white and resembles marble. In fact it is called marble locally, and the country where it occurs is flat with a very thin covering of soil. At other places the sandstone is baked into a hard quartzite traversed by joints at right angles to the bedding planes. This causes it to break up into great rectangular masses with curious structures like castles, towers, pinnacles, and minarets. Round these fixed structures lies a débris of disintegrated blocks, which recall the lines of Sir Walter Scott,

> Crags, knolls and mounds confusedly hurled,
> The fragments of an earlier world.

It was in such a district as this that the writer found his first sample of rock crystal *in situ*. It did not spring from granite nor from a quartz seam. It arose from the sandstone itself. In a gaping seam between the horizontal beds of sandstone were a number of beautiful crystals, of which the largest might have measured two inches across between opposite faces. A huge vertical joint, which was being wedged open by the growth of shrubs, gave access to the seam. There it was evident that the crystal had been formed by the "stewing up" of the sandstone, for the beds below the seam all showed incipient crystallisation. The crystals were generally speaking vertical, although at the surface of the seam they often inclined at irregular angles. But the planes of incipient crystallisation were always vertical and traversed the sandstone at right angles to the bedding planes. The sandstone was very brittle and could easily be pulled to pieces along these planes of crystallisation. One crystal could be pulled out therefore which showed quite distinctly the transition from sandstone to crystalline quartz (Figs. 2 and 3).

This seam was too small to yield crystals of commercial value, but, having obtained a clue as to how crystal was formed, the writer examined many other similar situations, often many miles apart, and digging into the soil at the base of the huge sandstone masses discovered other small seams of crystal. The conditions everywhere were the same. The sandstone had at some period in its history been subject to great heat which had melted and recrystallised it in the form of rock crystal.

The next course naturally was to try and map out the seams on the country and try to find the direction from which the heat had proceeded which had caused these peculiar changes on the rock. Then it was found that the crystal seams all lay round and near the great diamond mines of Sopa and Guinda, so a rapid survey of these was made. The "diggings" are in the form of a fissure, a mile or more in length in the sandstone. The miners term it a placer mine and it can be dug up like a loose river deposit. But closer examination proves that it is not an alluvial deposit, and that the rock here is composed of schists and fine clays with numerous concretions of ironstone. This suggests that this was at one time a pipe of basic rock

similar to those of South Africa in which the diamonds are found, but that the weathering and chemical changes in this case had altered the rock into the metamorphic series of schists and concretions which are now found in the mine. The view that these mines (and similar mines in other districts) are volcanic pipes is not at all generally accepted, but the field evidence, the long vertical fissure in horizontally-bedded sandstones, gives strong evidence in its favour. Again the fact that the sandstone in the neighbourhood of these fissures has been "stewed up" and recrystallised seems to indicate that at some time in past geological history these fissures contained a molten rock magma.

Fig. 2. Fig. 3.

Fig. 2. Single crystal of quartz showing transition from sandstone. The clear lines through the sandy portion indicate the lines of smaller bedding planes.

Fig. 3. Group of crystals springing from the sandstone in one of the larger bedding planes. (Serra do Espinhaco.)

Other diamond mines on the Serra do Espinhaco were visited and the same general features found; massive jointed sandstone surrounding an elongated fissure filled with schistose rock in which the diamonds were found. But further search for crystal was impossible. Gastric fever, blood-poisoning due to insect bites and the depredations of the "jigger," and finally a touch of malaria compelled the writer to leave this wild country and return to Rio de Janeiro

Subsequently a trip to another crystal district, the Serra des Cristaes in Goyaz, was arranged. After three or four days of railway travel through the rich agricultural state of Sao Paulo and western Minas Geraes, Ypamery in Goyaz, near the end of the railway line, was reached. Thereafter the distance to Cristaes, about a hundred and twenty miles, was made on mule-back.

The country in Goyaz is an ancient plateau of denudation about three thousand feet above sea-level. It is covered with thick hard grass, low shrub and small trees, with palm groves and patches of richer vegetation in the water-courses. Jaguars, wild boar, deer, monkeys, snakes, ostriches, parrots, and toucans are plentiful. The only industry is cattle raising, but the cattle do not thrive on the hard natural grass, and the ticks and other insects cause disease and ruin the hides. It does not pay to raise more than three generations of the one stock and there is consequently a continual import of foreign cattle. English cattle, such as are shipped to the Argentine, do not thrive in central Brazil and the white Indian buffalo is by far the commonest stock. The soil is a thick red clay, the product of the disintegration of the early palaeozoic rock of which the plateau is composed. In the dry season it gives rise to clouds of dust, and in the wet season to indescribable slime and mud. Solid rock is very seldom exposed.

Fig. 4. Fig. 5.

Two of the famous "Mines" on the Serra des Cristaes in Goyaz.

For three days the writer and his party plodded steadily forward over this rolling three-thousand-foot plateau, finding, when lucky, some scant accommodation at the rough log huts of the cattle ranches. On the fourth day the ground rose so gradually as to be scarcely noticeable until a height of four thousand feet was reached. This was the Serra des Cristaes. One or two small blocks of sandstone, overgrown and nearly buried under a growth of shrubs and trees, were all that were left here to represent the great rectangular masses of sandstone which were so characteristic of the Serra do Espinhaco. The "cristal" mines were mere shallow trenches, often many miles apart, scattered over this four-thousand-foot elevation (Figs. 4 and 5).

The crystal here is found in precisely the same conditions as in Minas Geraes. The four-thousand-foot elevation is composed of sandstone which is evidently the

remains of a hard cap. It has been recrystallised by heat. The crystallisation has been much more far-reaching here than near Diamantina and thick beds of sandstone over a great area have been altered. The rock is easily pulled to pieces along the planes of crystallisation and the writer in a few hours' work excavated a shaft about six feet deep and four feet diameter and found the crystal form more marked, the deeper he went on. There was one great drawback to the crystal here. The sandstone is invariably red in colour and impregnated with iron and manganese. The crystal consequently is smoky or coloured and useless for optical purposes. It is mined chiefly as a precious stone, the principal colours being yellow and amethyst. The yellow variety is often termed topaz and is indeed difficult to distinguish from real topaz when it is cut and polished.

It is worthy of note that there is no sign of a volcanic pipe or fissure in the Serra des Cristaes such as were found at Sopa and Guinda on the Serra do Espinhaco. This does not imply that such a pipe may not exist, only that it has not been discovered under the thick soil. But, as we saw, the material of the pipe weathers more readily than the hard sandstone and alters readily to soft sericitic schist and clay soil, so that a pipe on the Serra des Cristaes, where there are no deep river courses to lay it bare, might long remain unnoticed under the soil.

The town close by is known as the Villa des Cristaes and consists of a single street of rough wooden houses. The "store" is run by a couple of Germans who buy crystal from the native labourers who work it. More generally it is a case of bartering goods and provisions for it. The mines belong to no one and anyone is free to dig crystal as they will. There is a state tax of about fourpence-halfpenny per kilogram on all crystal mined, but there seems no way of enforcing the tax.

Several mines were visited but they showed no prospects of crystal clear enough for optical purposes in any quantity. So after a couple of days on the Serra the writer returned to Ypamery and thence by rail direct to the coast at Santos, and by sea to Rio.

At various agents' stores in Rio the writer examined large quantities of crystal, but it was generally speaking of little or no value. Often it was smoky or coloured; invariably it was full of seeds, veins, and marks which rendered it useless for optical purposes. Especially was this the case with large crystals which moreover often showed "phantom" crystals, i.e. crystals within crystals like a Chinese box. The largest single crystal which the writer saw weighed thirteen hundredweight and came from the Serra do Espinhaco some distance south of Diamantina.

The general impression of the writer is that though there are great quantities of so-called "cristal" in Brazil there is very little of any real value for optical purposes. The crystal everywhere seems to be a metamorphosed sandstone and only in a few isolated localities are there small quantities of crystal pure enough to be of commercial value. The crystal for optical purposes must be carefully selected by an expert. Before the war the Germans had trained mineralogists travelling in Brazil, who selected the crystal and precious stones and exported them to Germany or sold them to other countries. Since the war many Brazilian dealers in Rio de Janeiro have tried to capture the trade in rock crystal, but as they do not understand

it they have practically ruined the industry. They have had brought down from the interior large quantities of crude recrystallised sandstone full of impurities, seeds, and veins and fit for nothing better than road metal. Yet for this rubbish they brazenly ask a price of ten shillings per pound or even more.

The writer picked up many precious stones in his travels in Brazil. These included aquamarines, beryls, tourmalines, topaz, and coloured quartzites (amethyst and cairngorms). These were found lying on the surface of river gravels and were not searched for. There is a big commercial opening for mineralogists in central Brazil and it is to be hoped that England will not allow the immense resources of this country to be exploited by Germany alone in the future.

REVIEW

Visual Optics and Sight Testing. By Lionel Laurance, 2nd edition (The School of Optics Limited, London, 1920; pp. xii + 420, diagrams 156. Price, 21*s*.)

This is a large book of 420 pages and as the diagrams are relatively few, the letterpress is of imposing length. It is well printed and is remarkedly free from printers' errors, though "lagophthalmos" is wrongly spelt on p. 247. Two bad slips occur on pp. 7 and 227 respectively. In the former the latin equivalent of "optic nerve" is given as *porus opticus* and in the latter, headache at the top of the head is stated to be termed *supra-orbital*.

The author is very gentle to the mathematical tenderfoot. He invariably translates into a definite example every general statement; e.g. on p. 104 it is stated that "the axially H. eye is shorter than the Em. approximately ·33 for each diopter of error; *thus in H. 3 D. it is shortened 1 mm.*" On p. 139 the principal meridians of astigmatism are stated to be at right angles—then comes the example—"*For instance, if the one is at* 40° *the other is at* 40° + 90 = 130*; if the one is at* 175° *the other is at* 175 − 90 = 85° *and so on.*" Though in mathematics the author has considered the beginner, he has not always done so in his use of technical terms. In a large number of cases he has employed expressions without explanation which must bring the beginner to a halt because of their unfamiliarity. For example, on p. 4 the suspensory ligament of the lens is stated to arise from the membrana limitans interna, but it is only after two pages that the explanation of the term is given. The "muscle chart" is referred to on p. 94, but is only fully described on p. 173. On the same page the Placido disc is mentioned; its description is not found until p. 156. "Astigmatism against the rule" is referred to on p. 116 but not explained until p. 142. The term "prism diopter" is also used without previous explanation. It is true that frequently the direction (*q.v.*) follows a term used for the first time but the page is not added. The inquirer is left to search in the index for it. To a student who has attended a course of lectures and uses the book as lecture notes to confirm his knowledge, this criticism does not apply, but for a beginner some revision in the above sense would be advantageous.

Coming now to the matter expounded, the author has got together a large amount of information on the subject of the eye, but we think its value would have been increased if he had adopted the usual course and given references to his authorities. Though the mass of the information is sound, there are several instances of obscurity and statements of doubtful accuracy.

The following may serve as examples of what we mean. On p. 107, in the same paragraph, occur the following, "So rarely is the Amp. of Ac. below that assigned to each age that," etc., etc., and "the amplitude is liable to vary considerably in different individuals of the same age." On p. 11 it is stated that the conjunctiva "is prolonged over, and firmly attached to the cornea, forming its front layer." Now while it is enormously important to remember from a pathological point of view that the anterior layer of the cornea is *continuous* with the conjunctiva, in the same way as the substantia propria is continuous with the sclerotic and the endothelium on Descemet's membrane is continuous with the uveal tract, yet there is no more reason for calling the front layer of the cornea conjunctiva,

than there is for calling the conjunctiva skin, of which both it and the anterior part of the cornea are modifications. P. 98 reads "True Em. is so rare that excluding those actually myopic, practically all eyes are hypermetropic." Our experience differs entirely from this and we agree with Donders that "the majority of young eyes are undoubtedly emmetropic" and we go further and say the majority of eyes at the beginning of the presbyopic age are emmetropic. On p. 105 speaking of hypermetropia, "If *V* is subnormal lenses must improve it." It is true lenses generally do, but who has not met cases of uncomplicated hypermetropia in which no improvement of vision resulted from any lens? The statement, later, that V.A. is *generally* rendered normal is not strictly accurate; such a large number of hypermetropes fail to reach 6/6 with any lens.

On p. 111 is found "A person H. 4 D. would, at 20 years of age, be satisfactorily treated with, say, + 2D lenses; at 30 or 35 he would need, say, + 3D; while at 40 or 45 he would require + 4D." We think the majority of ophthalmic surgeons would put such a person into the condition of a normal man at 20 by ordering him his full correction at once. On p. 113 comes the statement that "there are many exophoric hypermetropes who wear Cc. lenses with comfort." We were not aware of this, but should such consult us we should have no hesitation in recommending convex lenses, decentred if necessary. We are assured on p. 112 that esophoria is "the condition of the muscles usually found associated with H" and on p. 126 that exophoria is "the usual muscular condition in M." Orthophoria seems almost as rare as emmetropia in Mr Laurance's opinion, but as a matter of fact heterophoria is by no means a common condition among Britons, though certain American writers profess to find it very frequently in their country.

In the following passages Mr Laurance is on dangerous ground. On p. 246 he points out that subnormal V.A. may indicate "amblyopia, abnormalities or lesions left by disease which may or ***may not*** require medical or surgical attention." On p. 340 he says "If visual acuity is ***naturally bad***, i.e. if, with the best possible correction, it is very subnormal, such correction is very difficult to arrive at subjectively." The italics in both instances are ours. It would appear from these passages that he is suggesting that opticians in some cases take upon them responsibility which he deprecates elsewhere. However we have purposely left this thorny subject without comment and will say no more.

We have read the book with interest and do not know of any single book which aims at uniting such a varied collection of information about the eye and its measurement. Although we should hesitate in following Mr Laurance in all his deductions, the book will be found to be of value to students as an introduction to the subject, and to others as a book of reference. With respect to the latter its utility is enhanced by the inclusion of an excellent index.

J. H. T.

ABSTRACTS

"The Errors of Measurement in Range-Finders." By Dr Hans Schulz, *Z. f. Instrk.*, 39 (1919), 91, 124, 242.

The usual expression for the error of measurement in the case of a range-finder is

$$f = \pm \frac{e^2}{b.v.20000},$$

where e is the measured distance, b the length of base, and v the magnification of the instrument. For the accuracy of vision an angular value of 1/20000, corresponding to about 10 seconds, is fixed upon. The value of f given by the above formula is known as the "minimum error"; in practice a value equal to four times this minimum error is introduced and known as the "allowable error."

The accuracy of range-finding depends, however, on other factors than the ones just mentioned. The form of the object, the weather, the type of setting, and the personal equation of the observer are some of the things that affect the accuracy of measurement. Taking these things into account, the author develops the formula

$$f = \pm K(1 - d^{-e}) \frac{e^2}{b.v.}.\delta,$$

where K is a factor depending on the type of instrument, d is an absorption coefficient taking account of the weather, and δ is a factor which includes the effect of personal error and the change in the physiological limiting angle caused by the constitution of the object; in the best cases δ falls to the value of 10 seconds. If the object is in motion an additional factor $(1 + \lambda\omega^2)$ is introduced into the formula, where ω is the angular velocity of the object in the direction of the line of division in the field of view of the range-finder, and λ is a constant which depends on the kind of instrument and on the dexterity of the observer.

The value of 10 seconds, which from experience has been assumed to be the value of the accuracy of vision, cannot be considered as constant. From a consideration of the effect on the retinal rods and cones it is to be expected that the longer the object is (within certain limits), the greater the accuracy of setting should be, owing to the fact that a greater number of retinal elements are brought into action.

In order to test the effects of size and form of the object a number of experiments were carried out. Two black rectangles of variable height and of breadth 50 mm. were set up on a white background. They could be moved sideways relative to each other distances of from 1 to 5 mm. If observed with the unaided eye at a distance of 20 metres a displacement of 1 mm. would correspond to the physiological limiting angle of 10 secs. In order to prevent the measurements from being influenced by the observer's other eye, binoculars having magnifying powers of 6 and 8 were employed at distances of 120 and 160 metres respectively from the object, one of the objectives being in each case covered. In this way errors due to accommodation were eliminated. For each set of measurements at least 50 trained observers were employed. The percentage number of correct estimations, corresponding to various heights and relative displacements of the rectangles are given in Table I.

Table I

Observation with 6× *binocular at distance* 120 *metres.*

Height in mm. ...	10	20	30	40
Displacement 1 mm.	25	31	34	29
,, 2 ,,	35	50	56	55
,, 3 ,,	25	57	72	77
,, 4 ,,	28	63	78	82
,, 5 ,,	35	68	87	82

Table I *a*

Breadth of rectangle 50 *mm.*

10	20	30	40
·22	·28	·31	·26
·16	·24	·27	·27
·07	·19	·22	·17
·06	·09	·17	·24
·06	·14	·21	·19

It follows that, at least for the larger displacements, there is an increase in the certainty of observation with increase in height of the object.

When an observer endeavours to set for coincidence, as in range-finding, the errors of observation will in magnitude and distribution follow the laws of probability. The probability ·5 will then correspond to the limit of the region of certainty, and therefore to the magnitude of the "minimum error." Now, for a given displacement s the probability that a setting will be within the region of the displacement is given by

$$W = \frac{2h}{\sqrt{\pi}} \int_0^s e^{-h^2x^2}\, dx,$$

where h denotes the coefficiency of accuracy. The same formula holds for the probability that a large number of observers, for a given relative displacement of the rectangles, will estimate the displacement correctly. Table I *a* gives the values of h calculated by means of the probability formula from the results of Table I. It can be seen that for a constant value of the ratio of height to displacement the value of h is constant and that with increase in this ratio h increases up to a more or less constant value.

In the case of the rectangles of breadth 50 mm. the physiological limiting angle was found to be about 20 seconds, whereas with rectangles of breadth 10 mm. the value was about 14 seconds, which is in good agreement with Wülfing's result, namely, 12 seconds.

Observations were also made with objects in the form of gratings and with black rectangles having white lines down the middle. In the latter case it was found that for low values of the height the accuracy was less with the white centre lines than without, whereas it was greater for heights of 60 mm. or more.

Further observations showed that the accuracy of setting increases, the more nearly the partial objects are normal to the line of division. It was also found that the triple field type of setting (central strip) does not greatly increase the accuracy over that obtainable with the double field type.

The author develops empirical formulae connecting the coefficient of accuracy with the height of the partial objects and their relative displacement.

The conclusion reached by the author is that with the usual forms of objects the "minimum error" may be taken as 10 seconds, but with partial objects inclined towards one another at an angle of 45° the error increases to 13 seconds.

J. S. A.

"Apparatus for testing the Accuracy of Setting." By C. v. Hofe, *Z. f. techn. Phys.*, 1 (1920), 85.

The paper describes a method of measuring the accuracy of different types of coincidence setting, such as are employed in range-finders. The apparatus employed consists of a board, 1 metre long, at one end of which is mounted a framework for holding the objects to be brought into coincidence. The lower of the two objects is fixed while the upper one can be moved sideways, its position being recorded by means of a micrometer screw, the head of which reads to 1/100 mm. The observer sets the objects in coincidence from a distance of 1 metre. The objects were designed on the results of Dr Hans Schulz's experience with observations of errors of measurement in range-finders (see previous abstract). They consisted of short cylinders of 1, 5, and 10 mm. breadth, some of them being flat and others pointed at the ends. The common axis of each pair of objects, when set in coincidence, was in most cases vertical, but in three cases made angles of 75°, 60°, and 45° respectively with the vertical. Observations were made by four persons, two of whom were trained observers. The results showed variations with the different types and with the different observers. The mean result of all the observations was an error corresponding to 5·8 seconds of angle, while the smallest error recorded was one of 1·7 seconds. It was found that the error of setting increases with the angle between the objects and the vertical, and also with increase of the vertical separation of the objects beyond a distance which subtends an angle greater than the limiting value of the sharpness of vision, which is about 1 minute of angle.

J. S. A.

"The Relative Merits of Monocular and Binocular Field-Glasses." By E. P. Hyde, P. W. Cobb, H. M. Johnson, and W. Weniger, *Journ. Franklin Inst.*, 189 (1920), 185, 331.

This paper describes the results of an investigation undertaken by the Sub-committee of the American National Research Council. The work is divided into three parts, I. Non-Stereoscopic Acuity Tests under Daylight Illumination; II. Non-Stereoscopic Acuity Tests under Twilight Illumination; and III. Field Tests.

The point investigated in Part I was whether in the use of a binocular field-glass the two eyes reinforce each other to any material extent. The conditions were full daylight illumination, and the criterion was vision of detail of minimal size and maximal contrast in a test object located entirely in one plane normal to the line of sight. In the cases of two of the observers the eyes used singly showed differences in acuity of about 4 and 7 per cent. respectively. No decisive advantage was found in the use of the two eyes together over the use of the better one alone. In the third observer there was a difference in acuity of about 15 per cent. between the two eyes, and in his case the use of both eyes gave results about 4 per cent. poorer than the use of the better eye alone. The task of raising the glass to the eyes and maintaining it in its proper position was made identical in the case of the monocular and binocular glasses.

Part II of the work was carried out in an underground tunnel, the test object being illuminated artificially. Under illumination of twilight intensity observation through

a prism binocular showed the following differences between binocular and monocular use:

(1) With binocular observation the threshold is somewhat lower than with monocular observation with the better of the two eyes.

(2) When the test object is of such size that the judgments are preponderantly correct, the decision of the observer is arrived at in shorter time with binocular observation than with monocular.

(3) When the test object is very small and all or nearly all the judgments are indeterminate, these also are to some extent rendered in shorter time with binocular observation than with monocular.

(4) With sizes of test object such that both correct and indeterminate judgments appear in large numbers, the tendency is to spend more time in binocular observation than in monocular.

It would seem, then, not only that vision with the glass is better and quicker with two eyes than one, but also that the observer is readier or more willing, for some reason, to prolong his observations in uncertain cases when he is observing binocularly.

In Part III the following tests were made:

(1) A general daylight test, in which a single man was the object of vision.

(2) A twilight test, similarly planned, but carried out late in the day until terminated by darkness.

(3) A counting test, in which groups of men varying in number appeared at stated places.

(4) A timing test, in which the time required to get the glass from the place where it is usually carried and report on an object resolvable only with the glass was determined.

In addition, the opinions of the observers as to the relative advantages of the two types of glasses, and their reasons were noted.

The result of the tests was in favour of binocular vision, but the maximum difference was only something of the order of 12 per cent. of the total experiments. As far as cost and speed of production, and lightness are concerned the advantage is overwhelmingly in favour of the monocular. On the other hand the comfort of use, involving the relative freedom from eye-strain or fatigue in other respects, would play a part in determining the results of the tests; and it also figures in the opinions of the observers.

It is pointed out that a monocular of magnification 6·27 would show the same performance as a binocular of magnification 6·00, that is, an effective difference in magnification of about 4·5 per cent.

For details of the tests, tabulated results of observations, and a complete discussion of the results, reference should be made to the paper.

J. S. A.